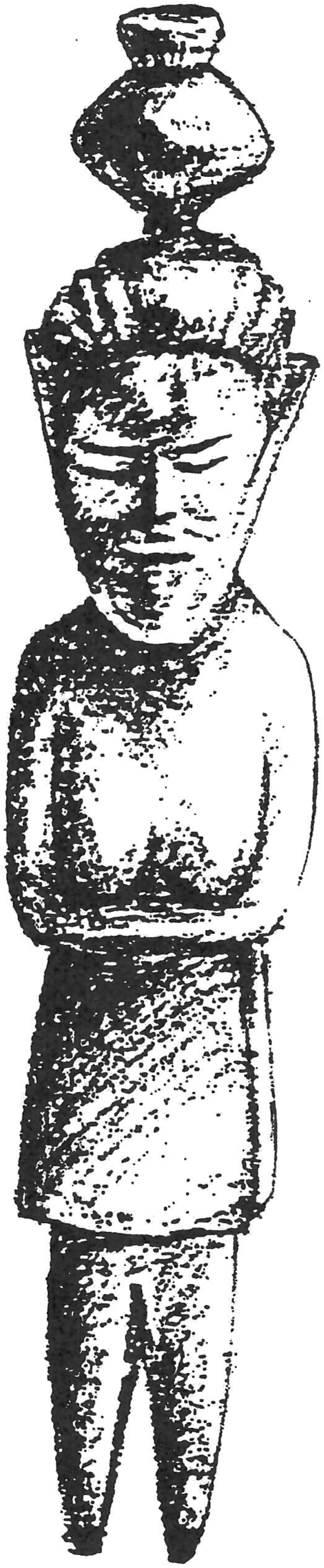
MOTHER TONGUE

JOURNAL OF THE ASSOCIATION FOR THE STUDY OF LANGUAGE IN PREHISTORY

Issue III December 1997



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TALCOTT PARSONS PRIZE IN THE SOCIAL SCIENCES

(Reprinted from the Bulletin of the American Academy of the Arts and Sciences, January 2, 1998)

At the Stated Meeting of the Western Center on December 6, Joseph H.Greenberg, professor of anthropology emeritus at Stanford University, received the Talcott Parsons Prize in the Social Sciences. The award was presented by Leo Beranek, president emeritus of the Academy.

The prize was established in honor of the distinguished sociologist who was president of the Academy from 1967 to 1971. Past recipients include Clifford Gertz (anthropology), Robert Dahl (political science), Robert Merton (sociology), Albert Hirschman (economics), C.Vann Woodward (history), and Daniel Bell (sociology).

The prize committee, chaired by Philip Converse, included Richard Epstain, Ernestine Friedl, Paul Kennedy, Richard Lampman (deceased), Stanley Lieberson, Nelson Polsby, and Stanley Schacter. The award citation follows:

The most appropriate backdrop for an understanding of Joseph Greenberg's achievements is the well-known reconstruction of the great tree of Indo-European languages, which dominates the developed countries of the West and stretches well beyond them geographically. This venture, conducted by a veritable industry of highly skilled linguists, has extended over a century. As a means of inferring how the original peoples lived, the systematic work moved, in its later stages, to a recreation of the proto-language as it stood many millennia ago, before fractionation into its myriad current language communities.

Greenberg has devoted his career to a similar venture aimed at reconstructing linguistic families and their roots in areas of the globe where such classification has been much more difficult. His first work, in the 1950s, reduced the indigenous languages of Africa to four macro-families. He later turned his attention to the comparative linguistics of the South Pacific (1971) and then, in 1987, published a parallel work that radically reclassified the indigenous languages of the Western Hemisphere into three main families.

This monumental sequence of works has been subjected to considerable controversy. There was a great deal of resistance originally to the African synthesis on the grounds that such deep-time reconstruction was impossible or was a product of sloppy methods. With the passage of time, criticisms have died out, and although details are still being adjudicated, the main lines of the structure Greenberg laid out nearly fifty years ago have now become the received wisdom. The Amerindian synthesis, now only a decade old, has also encountered harsh criticism that endures to this moment. It is impossible to judge the outcome of these disputes as yet. However, Greenberg's daring conjectures have recently received support from unexpected sources outside the linguistic community, most notably from physiological and genetic work. Studies of group variation in dentition in the Americas, for example, seem congruent with the Greenberg conclusions. On a much broader geographic canvas still, once large intervening population movements are taken into account, there is a reassuring fit between Greenberg's reconstructions of who was talking to whom several millennia ago, and the emergent evidence as to genetic distances between various subsets of human beings, as measured by DNA. Thus, Greenberg has provided one leg for a broader and more interdisciplinary architecture for inference about the prehistory of human populations.

Quite apart from the work on specifics of the world's language groupings, Greenberg has also made a steady stream of contributions to the generic study of linguistic typologizing and universals. And however much the details of his several geographical syntheses may require refinements in the years ahead, it seems clear that he has laid out an amazing series of first approximations. It is for this visionary performance that we find Joseph Greenberg deserving of the next Talcott Parsons Prize.

[**Editorial Personal Note**: Speaking for many scores of Africanists, including the vast preponderance of Africanist linguists, I can only shout `Hoorah! Way to go, Uncle Joe!'. After living with the Greenberg classification for 50 years in my case (since the first 1948 articles) and watching it get pruned but seeing it grow new branches, we can only contemplate the Americanist intransigence with amazement. Our continent has twice as many languages with vastly greater diversity in some areas (e.g., north Tanzania, Kordofan, southwestern Ethiopia) than their two continents. What is their problem? My answer is that clearly THEY are the problem. Demanding perfection, they miss the boat completely! As of 1998 they list 193 separate phyla in the Americas. Astounding, no?

One may perceive the reasons most clearly in the Americanist review of Lyle Campbell's new book, towards the end of this Journal. All the demand for rigor, all the worry about being wrong, all the need for absolute proof, all the concern for being fooled by inter-language influence and borrowing, all the things that keep a linguist mired in myopia, all that is visible there-in. I know the forest and the trees image is old and tired but still this seems to be a case where one can become a world class expert on a number of different **leaves** while entirely missing the **woods** where all the foliage finds itself. Maybe now it is time for another image. Short rangers are akin to **moles**, sans doute, while long rangers can claim the **eagle** as our totemic animal! **HF**]

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SPECIAL TOPIC: NIHALI

About two-thirds of the last issue of Mother Tongue (MT-II, December 1996) was concerned with the mysterious Nihali language of central India. It was our pleasure and privilege to publish there the most extensive collection of Nihali lexical material to date, including the first publication of Asha Mundlay's "Nihali Lexicon", as well as supplements from earlier reports. The volume also featured comments by eight discussants, representing a wide variety of views on the possible external connections (or the lack thereof) of Nihali.

The discussion continues in this issue with Paul Whitehouse's article, taking up as well another mysterious isolate of Greater India: Kusunda or Ban Raja. As explained by the author, this lengthy article is an outgrowth of his ongoing project of "systematic comparison of every cognate in every phylum with every other cognate in every other phylum." ASLIP/MT Founder Hal Fleming worked closely with Whitehouse in preparing the article for publication, and recommended that it be the centerpiece of this issue. So be it.

In "Nihali Phonemes", Roger Wescott proposes an explicit phonological structure for the Nihali language, which was not provided in MT-II. (See also "A Note on Transcription" in Whitehouse's article.) In the third article of the section, John Bengtson outlines some comments on a report by Ilya Peiros in MT-II. Attached to them is "Call it Methodology", a small critique by Hal Fleming of the strategy followed in the Peiros paper.

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The External Relationships of the Nihali and Kusunda Languages

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This survey of the evidence linking Nihali and Kusunda to other language families is based upon a systematic comparison of every cognate in every phylum with every other cognate in every other phylum. This comparative exercise is as yet unfinished in that I have still to use the forms put forward for every constituent family of every phylum, but is complete in that I have used the evidence published in support of all the highest level groupings, without exception.

Most papers of this kind include only those data that support the relationship the writer seeks to propose. The problem there is that, in the absence of a counter-argument, the strength of the evidence put forward is difficult to gauge properly. It **is** possible to find similarities between any pair of languages or language families -- I have done so myself -- and this fact will always be used against any set of 'pairwise' similarities presented in isolation. In order to get round this problem I have included every example that stands a chance of being cognate with the other examples, whether they support my preferred classification or not. This deliberate inclusion of weak data may appear counter-productive because, by obscuring the most telling examples, it obliges the reader to think for her- or him-self. However, the deficiency of some of the connections may actually reinforce the stronger connections, since weak data in one area implicitly support better data elsewhere by demonstrating the poverty of the alternatives. The presentation of comprehensive data also makes the absences meaningful; it permits no recourse to what may or may not be found elsewhere.

One final merit of this approach is that it offers clues to the deepest relationships of all, which I am convinced can only be glimpsed via global comparisons.

The external relationships of Nihali

Single, isolated languages are traditionally the hardest to classify since they offer so little to work with, and the more distant the relationship, the tougher the problem. Both Nihali and Kusunda fall into this category. With Nihali the problem is seriously aggravated by the obvious torrent of borrowings to which it has been subjected. Some of the examples that follow have been dismissed as borrowings from Korku, Marathi or Dravidian, but I have presented them anyway where I believe the direction of borrowing may have been from Nihali or an extinct close relative. It does not help that Mundlay in her article omits to say whether the supposed source of borrowing is unique to its family or has a wider distribution. Obviously, a word that is unique to Hindi or Marathi stands more chance of having been borrowed from Nihali than one that derives from an old Indo-European root.

These problems have not deterred linguists from attempting to link both Nihali and Kusunda to other language families. Bengtson and Blazhek's **Lexica Dene-Caucasica** includes Kusunda examples, while Merritt Ruhlen has also suggested Nihali words that seem to belong to Dene-Caucasic. Bengtson, meanwhile, has built upon similarities first remarked upon by Kuiper to add both Nihali and Ainu to a Macro-Australic grouping that includes Austric, Australian and Indo-Pacific. He stops short of positing a special relationship between Nihali and Ainu within this Macro-Australic, but this may be over-cautious. The evidence he presents linking Nihali to Ainu is better than any I have yet found for any other connection, Kusunda included.

The problem here is, of course, that Ainu has already been linked to various North Eurasian phyla (not least by Joseph Greenberg). I have not yet had access to all the evidence that has been put forward linking Ainu to Altaic, etc., but I have noticed that in many cases the `Macro-Australic' words in Ainu have synonyms or near synonyms which **do** seem to belong with the Eurasian phyla.

This suggests that Ainu may be a Macro-Australic language which has borrowed heavily from the Eurasian languages with which it has come into contact. Further research may reveal evidence of such remnants of a Macro-Australic sub-stratum in Japanese, Korean, Gilyak, Chukchi-Kamchatkan, Eskimo-Aleutian -- and perhaps even in Amerind.

As for Dene-Caucasic, one interpretation of the similarities to Nihali and Kusunda mentioned above is that both have been in contact with Dene-Caucasic languages and have borrowed from them. Within Dene-Caucasic, Burushaski shares more forms with Macro-Australic than do other sub-groups, which implies more recent additional borrowing. The alternative is a secondary relationship between Dene-Caucasic and Macro-Australic. Here the problem is that other evidence appears to place Dene-Caucasic with Nostratic and Amerind in a 'Borean' super-phylum which does not include Macro-Australic. Here at least we reach solid ground. Neither Nihali nor Kusunda is Nostratic -- and, whilst they exhibit a number of intriguing similarities to the various African phyla, they are definitely not African.

Personal Pronouns

Examination of the personal pronouns at once brings us face to face with this question. The Nihali first person pronoun 'I' fits so well into Dene-Caucasic that it is/was inevitable that the link should have been proposed:

`1st person singular'

NIHALI: dzoo ~ dzuoo; DENE-CAUCASIC: Caucasic: West *sV, East *zo; Burushaski ja, je, Yasin ẓ̌a; Athabaskan *ši, Yeniseian *ʔadz; KUSUNDA: ci ~ dzi; KHOISAN: Sandawe ci `1st p. pl'

This is, however, the first and only time that Dene-Caucasic pronouns figure in the comparisons. The presence here of Kusunda should be judged in the light of the more complete comparative set given in the second part of the article.

Nihali also has an oblique form `me' that is equally obviously Austric:

`1st person singular'

NIHALI: eŋ(g) \`me, my (1st sg. obl.)'; AINU: en [Batchelor], *an [Vovin] \`me'; AUSTRIC: Munda *iñ ~ ing; Mon-Khmer: Khmer añ, Mon ai, Bahnaric *ʔiñ, Semai (1) ʔing, Semai (2) ʔeñ \`I'; AUSTRONESIAN *-ŋku \`my, mine'.

The plural forms reinforce this:

\`1st person plural'

NIHALI: ṭeeku \`we(d)'; KUSUNDA: toʔi \`we(pl)'; AUSTRIC: Tai *tuu \`we (ex)'; W.Fijian *ti ~ tu \`we'; AINU: ci (<*ti-) \`we' (pl); CENTRAL NIGER-CONGO: *tí, tú \`we'; KHOISAN: Sandawe -so; South African Khoisan si ~ ši \`we (ex)'.

Quite what is to be made of the fact that Khoisan participates in two out of the three above, I am not sure. The 1st per. plural, meanwhile, points in so many directions that it merely confuses matters:

\`we (inc)'

NIHALI: manɛ , maaney \`we'; AUSTRIC: Miao-Yao *(m)pua; Munda *bi(n); Mon-Khmer: Bahnaric *biin \`we (inc)'; AUSTRONESIAN: Philippine *-mami \`we (ex)'; Oceanic *-mami \`our (ex)'; AMERIND: *ma \`we (inc)'; NOSTRATIC: Afrasian: Chadic *m(n) \`we' (inc)'; Dravidian *maa \`we'; Kartvelian *m- \`we (inc)'; Indo-European *me-s \`we'; Eskimo-Aleutian *wa \`we (inc)'; Chukchi-Kamchatkan *mur, *murx-; Uralic *mä- ~ me-; Gilyak me- \`we'; Altaic ba \`we (ex)'; Japanese baan(u); Ainu un \`us (obj)'; TASMAN: [NE, ME] mina, [SE] miina, [W]ma, [N] maŋa; ANDAMAN: Önge mii, m- \`1st pl.'

The second person pronoun \`thou', meanwhile, points equally to Australian, Nilo-Saharan and Indo-Pacific:

\`2nd person. singular'

NIHALI: nye / nye-ko \`thou/you-pl'; DRAVIDIAN: *ni- 2nd p. sg. & pl.; AUSTRALIAN: *nyin \`2nd p. sg.?pl.?; NILO-SAHARAN: Temein nin, Didinga -ni; Madi nyí; INDO-PACIFIC: Tasman *nina; Önge ŋi, Great Andaman *ŋu ~ *ŋo; Madang-Adelbert [NENG] *nA ~ *nɛ ~ *ni \`2nd p.sg.'; KUSUNDA nu; AFRASIAN: Nomotic: na, ne-na \`thou'.

The second person plurals bring us back to SE Asia:

\`2nd person dual.'

NIHALI: neeko \`you(dual)'; KUSUNDA: noʔi \`you pl.'

\`2nd person plural'

NIHALI: lá; DAIC: liu \`you(pl)' Lao lau \`thou'.

Cultural Indicators

What other clues are there? The Nihals must originally have been hunter-gatherers, and when hunter-gatherers are subsumed into the society of farmers or pastoralists and begin to adopt their languages, it is their hunter-gathering terms that are the most likely items to be retained. It is significant, therefore, that the Nihali words for 'arrow' and (less obviously) 'bow' are comparable to those in Jarawa:

NIHALI: badzo INDO-PACIFIC: Andaman: Jarawa bɟohi `arrow'; Rai Coast [NEBG] Sinsauru ubʌti, Asas ubʌdi, Sausi ibadi, Kesewai apʌti, Arawum bɟʌ:ku, Lemio batʌgu `bow(n)'

NIHALI: dʰanaako (Marathi dʰanušya); Andaman: Jarawa toŋi `bow'

The same also applies to botanical and zoological terms. Consider the following:

NIHALI: bʰer-dzoli `small red flower'; INDO-PACIFIC: Boven Tor berbere, Kwesten birbir, Monumbo purapura; Tasman: [NE] polo, buline, [ME,SE] pala; AUSTRALIAN: Nungubuyu bulbar, Djingili biʌyiŋbi-aga `red', Tiwi (yim)pulini 'blood'; AINU ɸu:re; KOREAN pʰal-gan; GILYAK (Sakhalin) paalant `red'; URALIC: E.Cheremiss bür; AMERIND: *pile `blood' [NB: unless stated otherwise, all reconstructions of Amerind and its subgroups are from Ruhlen 1994, based on Greenberg, 1987]; ?NIGER-CONGO: d.Ewe fɛfɛ, Gã fa:, fɛ: `red', Guang pɛ́, pɛ́lɛ́ `red, to be ripe, Ahlõ fɛ, Kposso vɛ, Sissala fía: `to be red'; AFRASIAN: *b-r 'blood' [Greenberg, 1963].
This seems a more likely source than Mundlay's suggestion that **bʰer** is from South Munda *bir- `jungle'; after all, not all jungle flowers are red.

NIHALI: tsarmaaru `yellow wild flower'; DENE-CAUCASIC: Yeniseian *suɾ - `yellow'; Basque zuri `white'; Caucasic *hʷö-c'ʷörʌ `grey, yellow'; AMERIND: Chibchan-Paezan *tara, Macro-Ge *tora; ALTAIC: Mongolian šari `yellow'.

Mundlay suggests that -maaru is from South Munda -mar `centipede, scorpion, stinging insect'. If so, this flower's true name is `yellow scorpion'.

It may of course be a coincidence that the Nihali names for red and yellow flowers correspond to 'red' and 'yellow' in non-Indian phyla, but I doubt it. These explanations, based on non-Indian comparisons, seem more plausible than Mundlay's `Indian only'. Note that one points towards Macro-Australic, the other towards Borean.

A Note on Transcription

I have tried as far as possible to use a uniform phonetic transcription throughout. The symbols -c- and -ɟ - are voiceless and voiced palatal plosives; -š- and -ž - are alveolar fricatives, -č - and -j- the corresponding affricates. Where I was unsure what -j- represented in the original source this is noted. I am also able to confirm that Mundlay's -c- and -ɟ - are alveolar affricates (as in Midlands English `hits' and `kids'), while -č - and -j- are the affricates in 'hutch' and `budge'. I know this because I rang her up and asked her. Such inter-continental phone calls are a welcome break from the solitude of taxonomic research, but in future can we include such important details in the text please?

I have encountered a number of statements to the effect that standardizing the transcription of data drawn from a variety of sources is insurmountably hard. Surely the advantages of using a single system, knowing that one is always comparing like with like, make the effort not only worthwhile but even obligatory.

Finally, when listing examples I have always left the shared meaning till last. The meaning for any given word will be the next meaning that follows it, which will be shared by every other word you pass on the way. Similarly, every word belongs to the language whose name immediately precedes it.

1. `all'

NIHALI: sagara `all of it'; INDO-PACIFIC: Andaman: Bea doga `much'; [TA] Abui toka; [NB] Taulil tugus; [BO] Siwai tuki; [WNG] Baham taghüa; [CNG] Dibiri tugurama; [NENG] Bongu jegar, Sungumana jagar; Asmat takas (ȷ̇=ž?) `all'. [NB: Here and elsewhere the abbreviations in square brackets refer to the Indo-Pacific sub-groups as defined in Greenberg, 1971. Subsequent research may require these to be modified, but for the moment they remain the most convenient source for locally characteristic forms.]

2. `anger'

NIHALI: k^{h}idzi ~ k^{h}idzo `to be angry'; AINU: ikešui `angry'; KHOISAN: Sandawe k›itl›e `to be angry'; TASMAN: [SE] kitreh `anger'.

3. `ant'

NIHALI: koköy `ant'; AINU: kiki(-)r `worm, insect, bug, fly'

4. `armpit'

NIHALI: kaṭhla; DENE-CAUCASIC: Caucasic: *q›w:ʌtl›:i[1] ; Burushaski -qʌt; Yeniseian *qoly; Tibeto-Burman *k(a)li; MUNDA: Korku kath(a)la, Mundari hatalaʔ `armpit'.

5. `to ask'

NIHALI: bitsá `to ask a question'; AINU: *pisi `to ask'; AUSTRONESIAN: Philippines *bitsara `to talk'; NIGER-CONGO: [Westermann] *bí (bítíá), Tschi bisa, Guang bisê; Nyangbo bisi; Bago bise; ?AMERIND: *matsa `to ask'. [NB: Westerman's Tschi is spelt Twi nowadays.]

6. `(tree) bark (1)'

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[1] Editor's note: in the original [q!wq!w] was written as a [q!^{w}] with a bar over the [q] to show gemination. The [tl!tl!] was written as [t$^{l!}$] with a bar over the [t] to indicate that the [t] was geminate or the whole glottalized lateral affricate was long. Some call this strong consonant `tense'. IPA uses [:] for length.

NIHALI: sali, ts^{h}ali; BASQUE azal `bark (of tree)'.

7. `(tree) bark (2)'

NIHALI: alaago `strip of bark used as rope'; AUSTRIC: Tai *pliak; Formosan *pulak; TASMAN: [ME, SE] puura(na), [W] puura(lea) `bark'

8. `bee'

NIHALI: ḍ^{h}eḍ^{h}ne `bumble bee'; BURUSHASKI: Hunza ḍonḍu `bee'.
[NB: See also No. 201 'honey, sweet'.]

9. `belly'

NIHALI: popo; MUNDA: Mundari pu'pu `belly', Ho pupuu `abdomen'; SINO-TIBETAN: Ladakhi p^{h}oa, Gurung, Mumi p^{h}o, Newari poaat^{h}a, Lepcha ta-bók `belly'.

10. `to bend (1); joint'

NIHALI: lappo (d.Marathi lap-ne) `to bend'; CHUKCHI-KAMCHATKAN: *ʔləp `joint'. [NB: Unless otherwise stated, proto-Chukchi-Kamchatkan forms are from Mudrak, 1990.]

11. `to bend (2)'

NIHALI: kotsa `to bend at an angle'; AMERIND: Almosan-Keresouian *kotun `crooked', Penutian *koc̆e `elbow'.

12. `to bend (3)'

NIHALI: ṭong-re `knee'; BURUSHASKI: Hunza ḍʊŋγʊraas; DRAVIDIAN: toŋk; INDO-EUROPEAN: *lenk-; URALIC: *loŋa `to bend'; GILYAK: Amur toŋk `elbow'; MUNDA: Sora duŋguu `to be bent', Kharia hinduŋ `to stoop, creep'; AUSTRONESIAN: Javanese deku, Malay tunduk `to stoop'.

13. `to bite'

NIHALI: tsapaṭ(a) `to chew'; AUSTRALIAN: Paman *paɟa `to bite'.

14. `bitter (1)'

NIHALI: kaago `to taste bitter'; INDO-PACIFIC: [SWNG] Boazi kagha, Ninati ko-ok, Metomka kok, Dumut koko `bitter'; DENE-CAUCASIC: Yeniseian *qʌqʌr `bile'; Tibeto-Burman *ka, Old Chinese *qaaʔ `bitter'; Burushaski γaaqay-um `bitter'; Caucasic: *qʼeqʼʌ ~ -eqʼa- `bitter'.

15. `bitter (2)'

NIHALI: khaata (?Hindi k^{h}aṭṭa) `sour'; KUSUNDA: katuk ~ qatu `bitter'; AUSTRALIAN: Tiwi malakati `sour', Margany gaṭi `salty, bitter', Mpakwati gaṭali `sour'; NILO-SAHARAN: Songhay hotta, Kanembu kuotu; Shilluk keec, Dinka, Luo kec, Acholi kɛɛc, Longarim akate `bitter', Didinga xaxat `to be bitter'.

16. \`blood'

NIHALI: tsorṭo, YENISEIAN: *sur; INDO-PACIFIC: [UNG] Warenbori da(-)ro [NB: loan word from Austronesian ?], Taurap sar; AUSTRONESIAN: *darah \`blood'; CHUKCHI-KAMCHATKAN: *ʔdzɚʔə \`red'; CAUCASIC: Hurrian zur-gi \`blood'; ?BASQUE: i-zerdi ~ i-zardi \`sweat, sap', if \`sap' (i.e., tree blood') is the primary meaning.

17. \`bone (1)'

NIHALI: gursa \`anterior tibial ridge'; ?SUMERIAN: gir; ?AUSTRALIAN: Yindjibarndi kur̲u[2], Nungubuyu ɳagar̲a \`bone'. Alternatively, this may also derive from Dolgopolsky's etymology No.4 in MT-II, p.62, via Dravidian *kurVtsV 'hoof'.

18. \`bone (2)'

NIHALI: pakoṭo 'bone' (Bhat.), tagaali paakto \`collar bone' (Mundlay); AMERIND: *paq' \`bone, gristle'; CAUCASIC: Abkaz-Adyge *pqqə \`bone' (see FN1).

19. \`to break'

NIHALI: petek- \`to break, to tear'; AINU: *pet-u \`to cut, split'; MUNDA: Santali pɛṭɛts \`to break off with the finger, nip off, snap off', Mundari peteʔ \`to break off a twig or small branch'; AUSTRONESIAN *bə̣ak \`to split'.

20. \`burn, be bright'

NIHALI: tshuluk \`to kindle'; DRAVIDIAN *tuɭ̥a \`shine, to spark'

The Dravidian example has been linked by the Nostraticists with Uralic *tule \`fire', Altaic *duli- \`warm, to warm up'; Chukchi-Kamchatkan *θəɬvə \`to burn (intr.)'. However, I suspect these may actually belong with No.34 \`smoke', via \`to smoke (tr.)' and the fires lit for this purpose.

21. \`bury, dig'

NIHALI: gadəw, gaadaay (Marathi gad-ɳ̣e) \`bury'; KADU: *igidana \`to dig'.[3]

22. \`buttocks'

NIHALI: kutsa \`rump'; DENE-CAUCASIC: Burushaski xʌšiɳ \`buttocks'; KHOISAN: Sandawe čʰina \`buttocks' (all from < *kušin ~ kutsin).

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[2] Editor's note: the symbol r̲ is used in place of an upside down [r] in the original which is used to denote approximants where these contrast with flaps and trills.

[3] Editor's note: Kadu is a new name assigned to the former branch of the Kordofanian sub-phylum of Niger-Congo. Kadu was formerly called the Tumtum group; it may represent a new phylum.

23. `cack'

NIHALI: g^hur `anus, excrete', $g^h$uurka `to go for a cack'; BURUSHASKI γʊraš; AUSTRALIAN: Maung gurag `cack'; BASQUE: khorotz ~ gorotz `dung'; CAUCASIC: *kʼurč̣ʼV `dung'

Trask argues that Basque **khorotz ~ gorotz** `dung' must be a loan word because it conforms to a rule that only applies to loan words. His reasoning is sound, but only as long as his division of Basque words into two categories holds good. His contention that one group consists only of loan words requires this to have first been established by other means, i.e., by showing that the loans all have plausible sources. However, since I do not accept that this is so (and I certainly cannot believe the **khorotz ~ gorotz** derives from Latin CROCEA `saffron-coloured'), I am forced to conclude that his distinction is not strong enough to support the theory derived from it, and this therefore cannot be used to dismiss the alternative proposal made by Bengtson and Blazhek.

My source for Basque, Aulestia's BASQUE-ENGLISH DICTIONARY, has **bekhorotz** `excrement, dung', which he states occurs in at least five dialects. My confidence in this source is not high (for instance, he sees no need to specify which five dialects), but if this citation were correct it would further strengthen Bengtson's case for the Basque **be- ~ bi-** class marker.

24. `to carry'

NIHALI: ṭutini `to carry water'; AUSTRIC: Indonesian *utsu$_{\eta}$ `carry (as a team)'; KHOISAN: Hadza tene; Auen [N] tane, tani, tere, !Kung [N2] tanni, tenne; Naron [C2] tani, tane, tene, Nama [C3] tani; /Auni [S4] tane, tani, tare `to carry'.

25. `child (1), son-in-law'

NIHALI: dzawaay (Korku dzaway, Marathi dzawai) `son-in-law'; NIGER-CONGO: WEST ATLANTIC: Kissi jua, Gola dzwa `child'

Note that this is a kinship term referring to intermarriage rather than blood relationship, which unites Nihali, Indic and Munda. And West Atlantic? Let's see the Y-chromosomal data first!

26. `child (2)'

NIHALI: nhanaa-ṭa `children'; OMOTIC: Hamar nana, Boro Shinasha nanaʔ-o-tsi `boys'.

27. `child (3), young, small'

NIHALI: d^hura `youngest person in group', edzer `boy'; DENE-CAUCASIC: Yeniseian *dzʌl `child', *tur ~ tul `new'; SUMERIAN: tur `small'; KUSUNDA: tala sai `son, boy'; KOREAN: t^hal `daughter', a-dil `son'; INDO-PACIFIC: Andaman: Kede etire, Cari etire `boy', Bogajib tire `boy, child', Juwoi tre, Kol -tre `baby; [TA] Oirata doli; [NB] Tauli idila `small, child';

[NNG] Kilmeri turi; [ENG] Uaripi, Lepu aturea, Mailu ature; Ulango (Rossel Island) tier `child'; ?AUSTRALIAN: Yindjibarndi waṭurri `girl'.

28. `chin, jaw'

NIHALI: oṭʰo `chin'; INDO-PACIFIC: [SWNG] Marind ete, Aghu te `jaw'.

29. `chop, cut'

NIHALI: tsekoṭo `to chop meat', tsekṭo `knife', tsilatak `to cut into pieces'; ANDAMAN: Pucikwar to-, Juwoi tɔke-, Kol -tɔke- `to chop'; AUSTRIC: Li *tʰak `to cut, hew', IN *təktək `chop off'; AFRASIAN: Ongota čʼak to eat'; Somotic: Ari čʼak- `eat with a knife, by cutting off bite size chunks while holding the meat in the mouth', Hamar čʼɪk `hack off pieces (e.g., meat)'; Nomotic: Male tiktik `cut to pieces'; Chadic: Peve/Lame dzak `cut in pieces', Gwandara tsugu ~ suku `eat meat'. [NB: These additional forms are somewhat different because their second consonant is a glottalic velar or was earlier. They may represent a second etymology.] Nomotic: Ometo dʼak' ~ dʼayʔ 'bite'; Agau: Xamta tsʼaayu `bite', Xamir tsʼakʼ `bite'; East Cushitic: Yaaku čʼaq-au `chew something'; Somali jaq- `to suck breast' 'to suck something', jiiq-s-o `suck noisily' (Arvanites 1991), proto-East Cushitic *čʼakʼ `chew, swallow' (Arvanites 1991)

30. `claw, (finger) nail; to scratch'

NIHALI: kertshi; Burushaski: Hunza qʌr ɛt; SUMERIAN: hur; GILYAK: xarp; AUSTRALIAN: Maung garg, Tiwi kər̲əɳai `to scratch', kər̲əmunaɣa `nail'; TASMAN: [ME,SE] kələgana `claw', [SE] rəgara `to scratch'; NILO-SAHARAN: Fur káarù; Ik kɔrɔk, Bagirmi koolo, Burun kɔrkɔn `claw'; KHOISAN: /Kam [S1] //kuru, //Ng-!ʼe [S2] //kuri, //Khrgwi [S3] //kɔla, /ʼAuni [S4] //kɔra, //Khʼau-//ʼe [N1], !Kung [N2] //kuru, !ʼO-!Kung [N3] //kulu, Naron [C2] //kʼɔro, Nama [C3] //goros `nail'.

31. `to clean'

NIHALI: mendan; INDO-PACIFIC: [NNG] Valman mentyi `to clean'; Monumbo mundigak(-tset) `to clean (literally `cleaning-make')'.

32. `to climb'

NIHALI: tsaako `to climb'; AUSTRIC: Miao *dzya `to ride'; Indonesian *t'akay `to climb', Polynesian *hake `upwards'. [NB: Indonesian *t' is taken from Benedict's AUSTRO-THAI LANGUAGE AND CULTURE; it is probably not glottalized.]

33. `cloud; smoke, dust'

NIHALI: dhulla `dust'; NILO-SAHARAN: Koman: Gule dyurret; Songhay (Gao) dullu `smoke', dula `cloud'; Fur suru `dust', suul (?<*sull Note the alternation elsewhere between VVC and VCC)[4]; Kunama duuda ~ dulluda

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[4] Editor's note: just to clarify this a bit more. It is a commonplace among field workers that long vowels are often written for

`smoke', daalama ~ dallama `cloud'; Nile Nubian tulli, Dinka tol, Nuer tuol `smoke'; NIGER-CONGO: Chakali taal, Mo tala, Bwamu dũduule `cloud'; KORDOFAN: Jomang d-ɚi(ŋ), Tocho d̪-a:ṛú `dust'; KADU: Miri t̪uruuruk, Tolibi (Katcha) t̪uruuru 'fog'; ?SHABO: tɔ:ru (?<Majang tuur < proto-Sahelian *toor) `smoke'; KHOISAN: Nama [C3] tsɚa, (?Sandawe tlaraŋ) `dust'; AUSTRALIAN: *dyur̲(u) `smoke'; MON-KHMER: Khmer t̪uli, Koho got̪ul; SINO-TIBETAN: *dul ~ tul `dust'; KUSUNDA: duliŋ `cloud'; AINU: urara `fog'.

The primary here appears to be *DYULU `cloud'. In some cases partial reduplication has extended this to DYULULU (of which the geminated forms -- DYULLU -- are contractions), referring to clouds of an obtrusive nature, and thence to the substances of which the clouds are composed (`smoke', `dust', `fog'). In others the shift has occurred without gemination. In Fur `smoke' is derived from `cloud', which has itself then shifted sideways to `dust' in order to accomodate a new word for `cloud'.
In Kusunda -ŋ sometimes appears to denote indivisibility (taŋ `water', ganigiliŋ `fog', pioŋsoŋ `light', dziŋ `oil', giloŋ `forest'. No, I cannot account for dibiʔoŋ `hill' or yəŋsoŋ `tail'. Alternatively, its ubiquity as an adjectival ending suggests that perhaps this also denotes nouns that are `self-descriptive', in the sense that water is watery, fog is foggy, light is light -- and clouds are cloudy.

34. `cold'

NIHALI: hiim `cold (adj.)'; Burushaski: Hunza ɣamu `frost'; AUSTRIC: Tai *hmuay `snow'; Indonesian *hamuɣ `dew'; INDO-EUROPEAN: Sanskrit hiima `snow' < P-IE *gheym-o- is a credible source.

35. `to copulate'

NIHALI: ṭaak^h:otsu `coitus'; BURUSHASKI: Yasin d-kʌt- `to copulate'.

I suspect this may be another loan word arising from intermarriage.

36. `to cover'

NIHALI: dziki-kap-ri `eyebrow' [Bhattacharya], `eyelid' [Mund-lay]; AINU: sik-kAp `eyelid', kap `skin, bark'; CAUCASIC: Dagestan *kabe ~ kabu ~ kabts; Burushaski: gap `hide (skin)'; AFRASIAN: *kp- ~ qp- `to close, cover'; URALIC: *kopa `bark'; DRAVIDIAN: *kapp- ~ kavɚ `to close'; ALTAIC: *k'apa- `cover'; ESKIMO-ALEUTIAN: Greenland Eskimo qapuk `scum, froth'; KOREAN: Middle Korean kĕpcil `bark'; JAPANESE: kabur- `to put on, to cover', kapó `bark'; GILYAK: xip `birch bark'; AMERIND: *qʼapʼa `cover,

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commonplace among field workers that long vowels are often written for misheard long consonants, and long consonants for misheard long vowels. In Kafa of Nomotic which has long and short vowels, and long and short consonants, this type of field error is serious. Such field errors are not due to sloppiness but rather to the difficulties of hearing the distinctions.

close'; AUSTRIC: Miao-Yao *kop `hat'; Kam-Sui *kùp `close, cover'; Indonesian *taŋkub `cover with a lid'; INDO-PACIFIC: Andaman: Kede kapo, Cari kaba; [NNG] Maprik, Wosera, Manambu cəpə Yelogu cəfi, Nyaura cəmbə `bark'; [SNG] Karima kaibo `skin'; [CNG] Kiniageima, Jalimo Anggapuruk (na)kap `(my)skin', Jupna gap `skin'; [ENG] Namau (iri)kape `(tree)skin, i.e., bark'; NIGER-CONGO: BANTU: *-koßa 'skin'.

In addition to the Indo-Pacific shown above, Greenberg includes Bunak koma and Baitsi kamu, which suggest an original form *KAMBE. If so, the above may also include KHOISAN: Hadza //k››uma, //k››ume; //Kh›au-//›e [N1], !Kung [N2], O-!Kung [N3] !kumma `to bury' and NILO-SAHARAN: Koma (Modin) kum-kin `to close'; Songhay: Saharan [sic] gum `to cover'; Nera gömme, gümme, Dinka, Shilluk kum, Nuer kwom, Lango gyum `to cover', Dongola kom `envelope', with the original meaning being `to cover'.

37. `to cut'

NIHALI: khandi, ganda; INDO-PACIFIC: [NNG] Sentani akənə Monumbo kinat, Apris akanani `to cut'; ?NILO-SAHARAN: SONGHAY: ka `to cut, slice'; NIGER-CONGO: *ká- `to cut', Bambara ka ~ kã `cut, shear', Mano, Mwa kã, Bantu *kara ~ kanta `to cut'.

38. `to dance'

NIHALI: tsana; KHOISAN: //Kh›au-//›e [N1] ʗane, !Kung [N2] ʗxane; Naron [C2] ʗàne `to dance'.

39. `day, sun'

NIHALI: dewtaa (Marathi devataa, < Sanskrit divaa) `sun'; INDO-PACIFIC: [WNG] Mairasi arawo, Moi dewe, Kampong-Baru tio; Andaman: Cari diiwu `day', diiu `sun'.

This is automatically assumed to be a loan from Sanskrit. However, the Indo-Pacific forms suggest that the Nihali might just as easily be an assimilated form of a pre-Indic word.

40. `to decay'

NIHALI: tshu `to spoil'; KHOISAN: //Kh›au-//›e [N1] ʗouu; Tati [C1] ʗoro, Naron [C2] ʗ'ɔ: ~ ʗú:; Nama [C3] tsowá `to decay'

41. `dig, chop, hoe'

NIHALI: koplya `hoe'; ANDAMAN: Bale ar-koop-, Bea ɛr-koop- `to dig'; AFRASIAN: *kHp `dig, chop, cut'; KARTVELIAN: *k›aap- ~ k›eep- ; INDO-EUROPEAN: *ke(h)p- `chop, dig'.

42. `do, make'

NIHALI: kor (Marathi kar) `to do'; KHOISAN: //Kh›au-//›e [N1], !Kung [N2] kuru; Naron [C2] kúru, Nama [C3] guru `to make'.

43. `dog'

NIHALI: sitaa; MUNDA: Korku tsita, sita, Kherwari seta; AINU: seta ~ sita \`dog'.

44. \`to drink'

NIHALI: ḍelen ~ ḍeleŋ; AUSTRALIAN: Djingili ḍara, ḍir-i \`to drink'.

45. \`earth'

NIHALI: khara; AUSTRALIAN: Djingili garalu, Tiwi kəluwar̲ti; ANDAMAN: Bale gɑarʌ-dʌ, Bea gʌrʌ \`earth', also Juwoi päkʌr-lekile, Kol pɛkär-ce \`earth' may belong here too.

46. \`egg'

NIHALI: kallen; INDO-PACIFIC: Laumbe keru, Savosavo kolei; AUSTRIC: Kam-Sui *krai; Miao-Yao *qyaw < *qlaw \`egg'; Polynesian *kala \`testicles'; NILO-SAHARAN: Berta gogolo; Mittu kele; Fur kilo; Songhay: Gao guri, Djeerma gunguri; NIGER-CONGO: Mandinka kili, Vai keri, Bisa gur, gyir; Dagari jɛli, Kusal gɛl, Moore gyellé; Bete gú, Gidie gì; Bantu *gìdí \`egg'; AFRASIAN: Also see Greenberg's Afroasiatic \`egg' (no.28, 1963)

47. \`extend, stretch'

NIHALI: taŋoi \`stretched'; AUSTRIC: Tai *tiŋ, Li *diiŋ \`stretch out'; Indonesian *γəntəŋ \`to stretch'; AFRASIAN: *t($^{h}$)ən ~ t($^{h}$)an \`extend, spread, stretch, endure'; DRAVIDIAN: *taṇ- \`to increase, thrive, abound'; INDO-EUROPEAN: *t($^{h}$)en- ~ t($^{h}$)on- ~ t($^{h}$)n̥ \`to extend, spread, stretch'.

The Afrasian reconstruction is Bomhard's, as are the Dravidian and Indo-European, all derived from his proto-Nostratic *t(^{h})añ ~ *t(^{h})əñ . I suspect Hal may not go along with the Afrasian. Either way, the Nihali looks closer to Austro-Thai.

48. \`eye'

NIHALI: jiki-r \`eye', dziki-kaap-ri[5] \`eyelid'; AINU: siki \`eye', sik-kap \`eyelid'; INDO-PACIFIC: [SNG] Barika, Dugeme, Peremka, Wandatokwe, Tokwasa, Mani, Kanum si, Ngowugar ji (j-=?); [SWNG] Marind idih, Kaeti itigio, Dumut eteko \`to see'; MUNDA: Juang je(tej) \`mote, sand in eye'.

49. \`face, cheek'

NIHALI: gaal (Marathi, Korku gal) \`cheek'; SHABO: kʼaleega \`chin'; KHOISAN: Sandawe kálě: \`face'; SINO-TIBETAN: *Kal \`jaw, cheek'; NA-DENE: Eyak l-quhɬ \`cheek', Haida qúl \`forehead'.

50. \`fall (1), sink'

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[5] Mundlay specifies [-ri] as a bound form, a personalizing suffix. Clearly associated with body parts, it could also be called a noun class marker. Final [-r] in 'eye' is probably a variant of [-ri]. The occurence of final [-ro] in many Warenbori forms is very suggestive of cognation.

NIHALI: buuḍi `to set(of sun)'; ANDAMAN: Aka-Kede o botɑ , Bale pɑatɑ, Pucikwar boodo, Juwoi boote-, Kol -booten, Bogajib oŋ bodɑkʌn `to fall'; AFRASIAN: *pdH `to fall, lie down'; Somotic: Dime fot `to fall'; DRAVIDIAN: *paṭʌ `fall, sit, lie down, lower oneself'; INDO-EUROPEAN: *ped-; AMERIND: Penutian *peta `to fall'

51. `to fall (2)'

NIHALI: čerk(o); JAPANESE: očiru-; AINU: hačiri `to fall'.

52. `far (1), long'

NIHALI: lamba; (?Hindi lamba) `long'; TIWI: karampi, TASMAN: [NE] ləmbe, [ME] malumbo, [SE] lom `far'.

53. `far (2), remote'

NIHALI: how `remote'; ESKIMO-ALEUTIAN: *ʔawa- `over there, far off'.

[NB: Unless otherwise stated, proto-Eskimo-Aleut forms are from Mudrak, 1989.]

54. `fear'

NIHALI: bakaa `to scare away'; BURUSHASKI: Hunza biik; AMERIND: Penutian *bʾuk `fear'. Ruhlen also links this with Macro-Panoan *pak `hate'.

55. `finger'

NIHALI: boṭo (Marathi boṭ, Korku booṭo) `finger'; ANDAMAN: Bale ooŋ-bɔ:dɑ `nail', Bea oon-bodo-dʌ `finger, nail'

56. `fire (1), hearth'

NIHALI: ápó `wood, to be lit'; AINU: *apɛ, Hokkaido ape, abe `fire'; Kurile apoi `hearth'; INDO-PACIFIC: [SNG] Kibiri ebani, Tumu ibani, Dorro beŋgi, Nenium bonja, Nombuic báš, Mol-e benji, Jei ben-beŋ `fire'; AUSTRALIAN: Paman *Ciipuy `smoke'; BURUSHASKI: pfu; AMERIND: Almosan-Keresiouan *pay; Central Amerind *pa `fire'; Algonquian *penkwi `ashes, powder', *apwaani `to roast'; Penutian: Chontal apíʔ `ashes', Hokan: Chimariko ʔapu `fire', Kiliwa ʔawphóy `smoke'; AFRASIAN: ʔəf ~ ʔaf `burn, be hot, bake'; Omotic *bend- `ashes', Somotic: Dime ʔaf `burn', ap-s- `make fire'; INDO-EUROPEAN: *Hep(h) ~ Hop(h) `to cook'; GILYAK: phinay `to cook', pliŋγ `ashes, soot'; CHUKOTAN: *ʔəpaŋ `to cook', *piŋ-piŋ `ashes', *pinə `to burn, shine'; KADU: Kufo (Kanga) yi-fánɛ, Miri afánnê, Talla (Kadugli) afá:na, Tolibi (Katcha) kifínnê, Sangali (Tumma) kafɛánna `burn'; AUSTRIC: Mon-Khmer: Pear puy `tinder', Khasi dpey `hearth, ashes'; Austro-Tai: Kam-Sui *puy, N.Tai *vii, Austronesian *hapuy ~ xapuy, Atayal hapuniq `fire', pəhapuy-an `fireplace'; NILO-SAHARAN: Koman: Kwama pʾikʾin, Komo, Twampa pʾiŋ `ashes';

There seems to be a consistent contrast between APA `fire' and APANG `ashes, hearth, cook', the second presumably derived from the first. It is possible that the contrasting Afrasian and Omotic forms derive from this original split.

57. `fire (2), burn'

NIHALI: oṭṭi `burn, fire'; KUSUNDA: dza ~ja `fire'; DENE-CAUCASIC: Basque su; Caucasic *ts›ayi `fire'; Burushaski ši `fireplace'; Haida č›aaʔ `fire'; Sino-Tibetan *tsha `hot'; SUMERIAN: i-zi `fire'; AMERIND: *(o)ti `fire, hearth'; OMOTIC: *ets› `to burn'; DRAVIDIAN: *otʌ `to kindle'; INDO-EUROPEAN: *Het- `fire, hearth'; ALTAIC: *asa `to ignite', *ooti `spark, fire'; Gilyak t'a; Aleut ata, Korean tha `to burn'; KHOISAN: Hadza ts›o / ts›o-ko `fire', Auen tɔɔ, Nogau do, !Kung tɔ:, Hiechware joaa, Naron tau, tou(ša), Nama tsao `wood, firewood, ashes used as fire'; INDO-PACIFIC: Andaman: Bogajib, Kede, Cari at; Tasman: [SE] to, [W] toi; Timor-Alor: Oirata ada, Makasai ata, Bunak ɔtɔ, Abui ara; [WNG] Madik yet; [SWNG] Boazi te, Awin de, di, Madinava da; [SNG] Jab te, Melmek ete, Ipikol tai `fire', Goaribari taetae, Makieu atha `to burn'; [CNG] Tsaga te `to burn (intr), Jabi, Simori utu, Moni usa `fire';,[NENG] Nupanob ote `fever'; [ENG] Gebi ita `fire', Mailu odoado, Sogeri taite `hot', Mafulu ade `to burn', Kanamara ati, Neneba, Hagari uti `ashes'; Bougainville: Nasioi nta, tai `to burn', Konua eto `light a fire'; Kamoro, Mimi uta `to burn'; KADU: *issi ~ išši; NILO-SAHARAN: Koma (Buldiit) wutti, (Madin) woti, Uduk ot; Berti azzi; Maba uosi(k), Mimi (of Godefroy-Denombynes) su; Fur uto, udu; Meidob ussi, Gulfan ees `fire', Maasai isʊi; Kunama ito; Mangbetu osu `to burn'; Madi asi, Kreish ošo, Lega kasua, kasi, Lendu kazz `fire'; KORDOFAN: Tocho b-ʉ́jú, Rere w-údu-nnə Ebang i-d̲ú-nnu, Abul w-úd-i, Utoro yá-wud-ɛ́, Shirumba y-ʊd-ə , Tiro yá-úd-a `to burn'; NIGER-CONGO: Bambara, Dyula, Mano, Vai ta, Mwi, Susu te, Maninke tia; Grebo tɔ́; Santrokofi ɔ-tɔ́ , Akpafu ɔ-tɔ , Adele ɔ-taŋ; Igbo i-tari, Edo e-ta, e-tai `fire'.

What leaps out here is that the geminated forms are shared only by Nihali, Kadu and parts of Nilo-Saharan.

58. `fire (3); burn, to be hot'

NIHALI: aḍek `burn(intr)'; NIGER-CONGO: Bobo-Fing tɔgo, Bijago (Ankaras) muntúγɔ, (Wun) Núntuugô `fire', Limba a tô:gi, Bullom wɔ: tuk, Sherbro túk `hot', Kalabari ḍókinaa `burn (tr), Dogon tɛgɛ `burn'; KORDOFAN:Nding pɛ:déko `burn', t-ík, Jomang ḍ-ík, Tocho ṭik `fire'; NILO-SAHARAN: Fur toke `hot'; Nera tok ~ dok ~ dog `to be hot', Nyangea tak `fire', Kunama i-tokke `burn(tr), tokomà `hot'; AUSTRALIAN: Gudung toko; INDO-PACIFIC: Önge tuke; Marind tekav, Bian tekaf, Berik tokwa, Bonerif tukwa, Doso diki `fire', Pisa taki, Tanah Merah (2) toga, Sangke tige, Bo take `ashes'; MUNDA: Sora taage `to be hot, burn'; AMERIND: *tuke `to burn'; AFRASIAN: *t›kw `flame'; INDO-EUROPEAN: *d$^h$eg$^w$h- `burn'; URALIC: täγʌ(t); ALTAIC: *t'oga; GILYAK: t'uṟř `fire'; JAPANESE: tuk `ignite, catch fire, burn'

59. `fish'

NIHALI: tsán; SHABO: čaŋa `fish'; ?Korku: tsade `fish sp.'

60. `foot, leg'

NIHALI: k^{h}uri `foot, leg'; AINU: čikiri; SUMERIAN: gir; SONGHAI: Gao cä kora `foot'; Sino-Tibetan *k(h)riaj .

61. `for'

NIHALI: daaya; KHOISAN: /Kham [S1] taa; !Kung [N2] ta `for'.

62. `four, 4'

NIHALI: talpono `fourth' or (tal)pono `four(-th)'; AUSTRIC: Mon-Khmer: Bru pon, Alak poon, Hre, Chrao p^{w}on, Rongao phoon, Bahnar puon, Vietnamese bon, Muong pón, Nicobarese foan, Khmer buon, Tailang pon; Munda: Mundari upun, Korku upuun, Karia ipon, Santali poon `four'; AINU: Saru poʔon emko, Asahigawa pón émko `one quarter'.

Hal asks what [tal-] means. I was rather hoping he would tell me. I do know it also appears in [talaari(re)] `third'.

63. `full, to fill'

NIHALI: b^{h}eera (Marathi b^{h}ar-ṇe) `to fill (intr), b$^h$erya; BURUSHASKI: Hunza bil `full'; SONGHAY: bɛrɛndi; AUSTRALIAN: Wamdarang bir `full'; ANDAMAN: Pucikwar ɔ:kɑ-pɛrɛη; Juwoi ɔ:kɑ-p'reη, Kol ɔ:ke-p'reη- `to fill'.

64. `to gather, heap'

NIHALI: pūndza `to make a heap'; AUSTRIC: Indonesian *ʔimpun `to gather'.

65. `girl'

NIHALI: pirdzu; AINU: opere `girl'.

66. `to give'

NIHALI: bee; SUMERIAN: ba; AFRASIAN: Ongota biʔe; Chadic: Western: Hausa baa, Gwandara baʔa ~ bay `give', Maha baha `hand'; Semitic: Central: Arabic by^c (archaic) [6]; SINO-TIBETAN: bi, pin, pii, byin, bik, pe(e) `to give', proto-S-T *piaj = piay.

67. `to go (1)'

NIHALI: iyeer; SINO-TIBETAN: Mumi yar `to run'; SUMERIAN: ir; AUSTRALIAN: Tiwi -ur̲i-, Warndarang -ṛa-, Ngalakan ṛabo-, Nungubuyu r̲uma `to go'; AUSTROASIATIC: Munda: Sora jir ~ ir `to go away); Khasi wir, yaar `to escape', kiar `to evade, flee'; Khmer wier `to avoid, flee'; Bahnar juer, Stieng vuir `to go away'.

68. `to go (2), come'

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[6] The Arabic form was kindly contributed by Dr. Yoel Arbeitman of the Institute of Semitic Studies, Princeton, NJ. Thanks to him.

NIHALI: piy- `to come'; AINU: pa-yi `to go'; SINO-TIBETAN: Sunwar piu, Rai pia; Vayu pʰi, Bahing pii, Angami pʰi, pir, Boro fai, Lalung fi `to come'; DAIC: Tai *pai `to go', Kam-Sui *paai `to go, walk'; MUNDA: Gutob pi-, MON-KHMER: Semang Plus peh, Sakai Tanjong bʰi, bej; ?NIGER-CONGO: [Westermann] *bía `to come'.

69. `to go down, fall'

NIHALI: dzaalú `to climb down'; AINU: esoro `to descend'; AUSTRONESIAN: Oceanic *suru(p), *soro(p), *solo(p) `to descend, go down'; NILO-SAHARAN: Songhay (Djerma) zeri `to fall'; Maba usuri ~ zuri, Lotuxo isur(ok) `to go down'.

70. `grass (1), green'

NIHALI: tsʰaagaa `thorny grass sp.'; BURUSHASKI: Hunza šiqa `grass'; ?SUMERIAN: sig `green'.

71. `grass (2)'

NIHALI: booy (Korku boe) `grass, fodder'; AMERIND: Penutian *pu, Chibchan-Paezan *peu `grass'.

72. `hailstone'

NIHALI: gaargoṭi (Marathi gaar); MUNDA: Gutob kara pakan, Mowasi gara, Korku gara ~ gaara `hailstone'; SONGHAY: gaari `hail'; DENE-CAUCASIC: Yeniseian *gǝte `winter'; BURUSHASKI: kati ~ qati `hoar frost'.

73. `hair'

NIHALI: sika `pubic hair'; MACRO-CAUCASIC: Caucasic *čʼaHʌrʌ `hair'; BURUSHASKI: *čooqur-aŋ `fringe, forelock'; SUMERIAN: suxur[7] `hair of head'; MON-KHMER: Bonam sak, Khmer sãk, Alak sok, Muong thák ~ sák; SHABO: čʼeeka ~ šek; NILO-SAHARAN: Aja jɛkɛ; MANDE: Bambara si ~ siγi; Dyula (kun-)zigi, Mano ti ~ si, Kpelle (ŋuŋ)ḍeγa, Vai (kun-)di; KHOISAN: Sandawe tsʼe `hair'.

74. `hand; give'

NIHALI: ma; DENE-CAUCASIC: Basque e-ma-n; Archi mac `to give'; TASMAN: [W] reanemana `fist', [NE] anamana `hand'; AUSTRALIAN: Ngandi maŋa `hand'; AMERIND: *man- ~ mak- `to give'; INDO-EUROPEAN: *man ~ mər `hand'; URALIC: Yurak mana `finger'; ALTAIC: Tungus mana 'paw'; Korean manci `touch'; AINU: amojn `hand' (-j-=?), imeka `gift'; Gilyak imγ- `give', man- `measure by handspans'; Aliutor mənγ-, Kerek mənəgal `hand'; KHOISAN: Hadza ma; //khʼ au-//ʼe [N1] ma; Naron [C2], Nama [C3] ma `to give'; NILO-SAHARAN: Berti may; South Nilotic: Datog menek-t `hand'. See also No.225 `to take'.

75. `head'

NIHALI: pyeŋ `head', ONGOTA: bʼine `head, hair'; NIGER-CONGO: Bantu *boŋgó;

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[7] Editor's note: In the original this velar fricative was written as [h] with a curved line under it. We write that [x].

NILO-SAHARAN: Songhay boη-bánγò; INDO-PACIFIC: [NNG]: Wembi, Skofro mbaηger, Bosngun paηən, AUSTRALIAN: Tiwi puηintaγa; ?AINU: pe `head'; ?AMERIND *pe `forehead'

76. `hear, ear'

NIHALI: tsikn, tsakini; NILO-SAHARAN: Kunama -tii-, Ilit -tiika `hear';

77. `heart'

NIHALI: tsaawki `heart (of lamb)'; AMERIND: Hokan *tsukul; Chibchan-Paezan *soka `kidney'; NILO-SAHARAN: Kanuri tago; Berti sigi ~ jigi, Mursi taàgi `heart'.

78. `hook'

NIHALI: geri `fish hook; GILYAK: kherqηa `seal hook'.

79. `hot'

NIHALI: tsačuko `hot', čača-kaama (?*čačak-kaama, where [kaama] is a known causative) `to heat'; AINU: *sε:sεk `to be, grow hot'; AUSTRONESIAN *segseg `to burn'; East Oceanic *saka `hot'.

80. `house'

NIHALI: awar (Hindi aavaar `house', Marathi aavaar `compound area of house'); MUNDA: Korku ura, Mundari oṛa, Santali oṛak; AFRASIAN: ONGOTA wura ~ hura ~ ura `house'.

81. `hunger'

NIHALI: tsato, tsaato; KHOISAN: Sandawe tsʼo, tsʼootsʼi `to be hungry'; !Kung [N2] dzau, sau `to hunger.

82. `imperative (suffix on verb)'

NIHALI: -be; SINO-TIBETAN: Chatiya -be 'imperative suffix; INDO-PACIFIC: Önge -bε `verbal affix'. AFRASIAN: Nomotic: Gongan -be `imperative suffix' on verbs; AUSTRALIAN [CA] -bu- `to hit'; in compounds `to act upon', e.g., Ngalkbon gulaʔ `skin' > gulaʔ-bi `to skin'.

83. `interrogative'

NIHALI: nani `who?', naana `what?; DENE-CAUCASIC: Basque no- `interrogative'; Caucasic: West *nʌ, East *-an(ə)-`when?', Lezgi ni `who?'; Burushaski ana `where?, whither?', Yeniseian *ʔan- `who?'; SINO-TIBETAN: Tibetan na `when?'; KUSUNDA: nataη `what?, anak- `where?'; DRAVIDIAN: Parji naη `why?', naa(to); JAPANESE: nani `what?' ; AINU: nen `who?', nep `what?'; AUSTRIC: Katuic *(nə) naaw `who?', Austronesian (Philippines) *nenu `what?'; AUSTRALIAN: *ηani `who?', Paman ηaani `what?'; SHABO: nee; NILO-SAHARAN: Daza nya, Maba nyia; Fadidja, Mahas na, nai, Kenuzi, Dongola ni, Didinga ηani, Nera na, nan, Nyima ηa, Merarit na, Nuer, Dinka, Anuak, Lango, Bari ηa, Nandi, Luo ηɔ, Karamojong, Lotuxo ηai, Temein ηε `who?'; Berta naanu, naano `what?'; Kunama na `who?, which?'; NIGER-KORDOFANIAN:

Swahili nani `who?'.

I am not sure this gets us much further forward, however.

84. `to kiss'

NIHALI [Kuiper]: ṭo; INDO-PACIFIC: Andaman: Jarawa itoli `to kiss'.

85. `language'

NIHALI: maanḍi; BASQUE: Labourdin, Lower Navarrese mintso, mintzaira `language'.

86. `leaf (1)'

NIHALI: pala `leaves of tree' (Marathi pala, Korku pula `leaf'); AUSTRIC: Miao-Yao *mbloŋ `leaf'; Tai *tooŋ `large leaf'; Indonesian *buluŋ ; Munda: Bonda paʔru; INDO-PACIFIC: Tasman: [NE] paroko, [W] paroce, [ME] perote; [New Britain] Uasi boloxu; [Bougainville] Koromira bara, Nasioi paana; [CNG] Sisiame, Pirupiru pori, Barika iboro, Gijara poringai, Kawam per, Komelon pur, Mombum epur, Koneraw bur; [ENG] Manokulu evarau, Suku, Koiari, Maiari, Koita hana; Mantembu bara; AUSTRALIAN: Tiwi pərar̲ti, Wamdarang mbil̥al `leaf' Bardi bilili `leaves'; NOSTRATIC: AFRASIAN: *ʔbl `leaf, growing plant'; DRAVIDIAN: *poli `to grow, bloom'; Indo-European *bhel ~ bleh `plant, leaf, flower'.

87. `leaf (2)'

NIHALI: tsokob `leaf of tree; clan name'; MON-KHMER: Katuic aso'q; MUNDA: Korku, Mundari, Santali sakom `leaf'.

88. `lip'

NIHALI: tev-re; ?INDO-PACIFIC: [SWNG] Pisirami tapar, Tagota taper `lip', Tirio diware ~ taware `mouth'.

89. `to live, alive'

NIHALI: ugaaen [Bhattacharya], ungay [Mundlay] `to live'; KUSUNDA: aigə `alive'.

90. `lizard'

NIHALI: duma; ?KOREAN: domabem; ANDAMAN: Bea təimʌ-dʌ, Kede, Bogajib tatʌm `lizard'.

91. `louse (1)'

NIHALI: kepa `louse'; AINU: kapo `nits'; ?KHOISAN: //Kh›au-//›e [N1] kama (c̆u); Tati [C1] !kam-//ama, Naron [C2] /kama, Nama [C3] (/kunutsi-)//koma `beetle'; AMERIND: Equatorial *koma `louse'; ?NOSTRATIC: AFRASIAN: *km ~ qm `louse, tick, flea' [Illych-Svitich] but proto-Semitic *q-m-l [Leslau]; Altaic *kömi- `louse, ant, beetle'; Eskimo-Aleut *kuma `louse', *quma `flea'; Chukchi-Kamchatkan *kəmaχa ~ kənχa `stinging insect'.

The Nihali and Ainu forms may or may not share common origin with the other forms shown above. Either way, they clearly stand apart.

92. \`louse (2)'

NIHALI: tsilar-ta \`lice (found in plural only); NOSTRATIC: Kartvelian *ṭil-, Uralic *täyʌ, Altaic *tsi; AMERIND *ti; AUSTRIC: Miao nčhao, Yao žo; Vietnamese, Muong či, Mnong Biat č'i; Chrao si; NILO-SAHARAN: Kunama tila; Dilling itid, Bari ciro(tat) \`louse'; Maba til \`louse, flea'.

93. \`many, much'

NIHALI: then \`much'; MON-KHMER: Rongao tidʌŋ, Bonam diʔdoŋ, Sedang didong \`many'. Önge wo-taŋabɛ \`many' may also belong here if -bɛ is the verbal affix referred to in NO.82 above, and the true meaning here is \`they are many'.

94. \`milk'

NIHALI: ḍuḍ ~ dud ~ dudu (Marathi dud, Korku didom) \`milk'; BURUSHASKI: Hunza ḍuḍuuro \`nipple'.

95. \`monkey'

NIHALI: sara, tsarko \`baboon, black-faced monkey'; AUSTRIC: Munda: Korku saara \`black-faced monkey', Sora karuu; Mon-Khmer: Serting terau, Jelai rau, Tembi raʔ, C.Sakai ro ~ rau; JAPANESE: saru; AINU: saro \`monkey'.

96. \`moon'

NIHALI: [Mundlay] tʰended, [Bhattacharya] tʰenḍey (although Mundlay does not see this as a Marathi loan word, Bhattacharya also gives Marathi tʰended); SUMERIAN: denzu \`moon'; KUSUNDA: jun [Hodgson] \`moon'.

Bengtson (personal communication) suggests that these are part of a much larger set -- within which they nevertheless constitute a phonetic and semantic subset.

97. \`mosquito'

NIHALI: dʰokanaan \`large mosquito'; ANDAMAN: Jarawa hohenaŋi \`mosquito'.

98. \`mouth'

NIHALI: kaggo; SUMERIAN: kag, ka. Also Paul Benedict's proposed link with proto-Tibeto-Burman *m-ka \`opening, mouth, door', although inherently less likely as a CV versus a CVC.

99. \`narrow, tight'

NIHALI: kʰeri \`tight'; ESKIMO-AKLEUTIAN: *qur<u>i</u> \`narrow'.

100. \`needle, sew'

NIHALI: tsʰim \`to sew'; KOREAN: ja-čim; ?AINU: kem \`needle'.

101. \`night'

NIHALI: méndi \`night'; AINU: *mɔn(-)re \`to be late at night'.

102. `no, not (1)'

NIHALI: beko `no'; SHABO: -be `negative affix'; NILO-SAHARAN: Kanuri ba; Fur a...ba; BURUSHASKI: bɛ `not'; ANDAMAN: Bogajib pɑi-e, Cari yɛr bat, Kede yiir pat, Bale yabɑ, Bea yabʌ, Pucikwar, Juwoi pooye, Kol pooyi- `no'; AFRASIAN: NOMOTIC: Ometo dialects -beʔ- `negative affix on verbs', Dorze baya ~ bawa `no'; Ometo opp- `negative affix for imperative and jussive'.

103. `no, not (2)'

NIHALI: beṭe, bet^{h}e, bidzi `not'; DENE-CAUCASIC: Yeniseian *ʔat ~ ta; Dagestan *tʌ `no, not'; Tibeto-Burman *ta `prohibitive'; GILYAK: ta, JAPANESE: de nai; AINU: utek; KOREAN :an-ta; KHOISAN: Sandawe tsɛ; KADU: *ṭá-; NIGER-CONGO: *ta `not'.

This would suggest either that Nihali **beṭe** is some kind of double negative or that one or the other set of cognates is not valid. Either way, this sheds a little more light on how negatives are distributed globally.

104. `old person (1)'

NIHALI: dakaari `(robustly) old; CHUKCHI-KAMCHATKAN: *θəxla ~ təla `old man, old woman'.

105. `old person (2)'

NIHALI: pat^{h}a `elder, old', paṭ$^h$yaa `old man'; INDO-PACIFIC: [WNG] Nafri batua, Murik patengo `old', Tanah Merah (2) petua, Mekei petüe, C.Sentani patia `old man'.

106. `one (1), only'

NIHALI: baḍaa ~ baḍa ~ beḍe ~ biḍi ~ biḍik ~ biḍiko `one'; AINU patɛk `only'; BASQUE: bat `one'.

107. `one (2), first'

NIHALI: tsayni `first', tseyni `first, previously'; AINU: si-ne, š^{h}ine, INDO-PACIFIC: [UNG] Warenbori iseno, Taurap neisano, Pauwi osxenu; ESKIMO-ALEUT: *ʔata; KUSUNDA: qəsədən, goi saŋ; CAUCASIC: *ts^{h}ə YENISEIAN: *χu-sa; AUSTRIC: S.Kelao tsi; E.Formosan *atsa, Tsouic *tsani, Oceanic *ŧsa; Khasi si `one', Pacoh saaŋ `first', Munda *sɛŋ `first, before'.

Greenberg uses the term [UNG] to describe those languages he considers isolated within Indo-Pacific. Ruhlen also says: "It is likely that the unclassified New Guinea languages are members of Indo-Pacific." Surely when the data on New Guinea are fuller or complete, we will be able to place the [UNG] groups more definitively.

108. `to open'

NIHALI: ugar- `to open; BURUSHASKI: Hunza -čaqʌrʌs `to open (intr); JAPANESE: akeru, AINU: čaka; ANDAMANESE: Pucikwar, Kol ɔ:cʌ, Juwoi oocʌk, Bale ɔ:co-, Bea ɔ:cai- `to open': (all from *čakar).

109. `outside'

NIHALI: bahare; YENISEIAN: *hɤ-; ?BURUSHASKI: hool-um `outside'

110. `path, road'

NIHALI: dáy `way, road'; ANDAMAN: Bale teeŋʌ-, Bea tiŋ ʌ-, Pucikwar taiŋ-, Juwoi, Kol taieŋ `path' (?*dayaŋ < *dalaŋ); MON-KHMER: Muong tàŋ xá, Vietnamese düöŋ, Mnong Biat, Bahnar, Mnong Gar, Pongao troŋ, Alak ntooŋ, Chrao tsroŋ; KADAI: Thai *daaŋ; AUSTRONESIAN: Formosan *dzalaŋ, IN *dalan `road'; SINO-TIBETAN: *-roŋ `road'.

111. `to place'

NIHALI: ṭokaa `to place on'; ANDAMAN: Bale tɛg-, Bea tegi-, Pucikwar, Juwoi, Kol tɛc- `to place'.

112. `to play'

NIHALI: kʰyaalaa; (Marathi kʰyaḽ̊); SONGHAY: Gao korɛy; AUSTRALIAN: Ngalakan gaḽ̊uk; ANDAMAN: Bogajib -kole, Čari -kolɛ, Pucikwar, Kol -koole `to play'.

113. `plural'

NIHALI: -ṭa; AFRASIAN: *-aat; KARTVELIAN *-t; URALIC *-t, ALTAIC *-t `plural'.

The options for plural marking are so narrow, however, that I would be surprised if this were not much more widely distributed -- and consequently much less meaningful. If the options for plural marking were not actually narrow -- in simple empirical terms, then the [t] plural marker would become much more valuable, i.e., meaningful.

114. `to pull (1)'

NIHALI: kʰeri(-kama); AUSTRALIAN: Malakmalak kur, Ngalakan gar-bu `to pull'.

115. `to pull (2), strip'

NIHALI: oriḍ `to strip'; AUSTRIC: Mon-Khmer, Bahnar hrot `to pull lice out', Tai *ruut ~ root; Formosan *misurut, Indonesian *urut `to pull lengthways'.

116. `root (1)'

NIHALI: ḍibi `poisonous root'; AMERIND *tap; AFRASIAN: Nomotic: *ts›ab› `root'.

117. `root (2)'

NIHALI: baabaraa `edible root'; KADU: Talasa (Tumtum), Miri b›árrɛ́; INDO-PACIFIC: [WNG] Mairasi ɸara, Kapaur, Baham pur, Moraid pere `root'.

118. `root (3)'

NIHALI: ḍotaako `edible root'; KUSUNDA: itak [Reinhard & Toba], itaaq [Reinhard 1976]; ANDAMAN: Bo cok-, Kol, Pucikwar, Juwoi cok-, Bate, Bea coq; AUSTRALIAN: Tiwi: thikarla [8]`root'; ESKIMO-ALEUT: *tsuqi; CHUKCHI-KAMCHATKAN: *idzək `edible root'.

119. `rotten (1), pus'

NIHALI: tsherekka `pus'; CHUKCHI-KAMCHATKAN: *rəqə `rotten'.

120. `rotten (2), pus'

NIHALI: olki `to have fungus'; GILYAK: lolq-; CHUKCHI-KAMCHATKAN: rqəyəl `pus'.

121. `to rub, wipe'

NIHALI: uḍi `to rub'; ANDAMAN: Aka-Bale idi(-rɑar) , idi(-rar) `to wipe'.

122. `to run'

NIHALI: tsergo `to run'; NOSTRATIC: Dravidian *tsoṛ- `to run, flee'; Eskimo-Aleut *ts(i)rak `to have diarrhea'; NILO-SAHARAN: Gao zuru; Kunama ijari; Koma (Madin) zerri 'to run'.

123. `saliva'

NIHALI: pep^{h}-ry-a: `saliva' (Marathi p$^{h}$epre `saliva' which may have the Nihali personalizing suffix [-ri]); AINU: *pôp `sweat'; INDO-PACIFIC: ANDAMAN: Cari ma pup, Kede ma bub `saliva', Cari, Kede akʌ-pup, Bea o pup-dʌ, Pucikwar o-puup-dʌ, Juwoi, Kol -puup- `foam'.

124. `salt'

NIHALI: tsopo; KHOISAN: //Kh›au-//›e [N1] dabe, Naron [C2] debe, Tati [C1] debe; AMERIND: Lenca ç›epe, sepe; DRAVIDIAN: *tsuppu; AINU: sippo `salt`; JAPANESE šuppai `salty'.

125. `scorpion'

NIHALI: neygo; ONGOTA: hanago `scorpion'.

126. `scrotum'

NIHALI: boṛa; ?SONGHAY: foro `scrotum; ?MANDE: Bambara fɔ́rɔ `penis'.

127. `to separate, divide'

NIHALI: phari(-kaamaay) to divide property; DENE-CAUCASIC: Basque ber(-hezi) `to separate, distinguish; district, region'; Sino-Tibetan *bra ~ p$^{h}$ray `forked, spread, scattered, disperse; gap'; Ket bar,(in-)bar `gap, interval'; SUMERIAN: bar `to separate, decide, open; half'; NOSTRATIC:

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[8] The [th] stands for a laminated stop in inter-dental position, written as [t] with a wee half circle under it in the original. In Australia our [th] is often written as [th], for which Dixon recommends a simple [dh], plus [dj] for the lamino-palatal.

Afrasian: Semitic *fr-q ~ *pr-q ~ pr-ṭ `split, separate, divide'; Dravidian *par- `split, tear, rend, separate', *pir `sever, separate'; Kartvelian *p($^h$)ritsʼ `tear, rend, break, burst apart'; Indo-European *per ~ por `allocate, separate, divide'; Uralic *pärä `to break'; Altaic *pʔöŕü ~ pʔüŕü `separate, divide'; AINU: pereke (<*pere-kama?) `to break, tear'; MANDE: Bambara, Malinke fara, Dyula faraŋ, Kpelle faɭ̥a; Cental Niger-Congo: Adele fáráŋ `separate'.

Incidentally, one may speculate that this cognate has something basic to do with the Ruhlen-Bengtson global etymology *pal for `two'. As in dividing things, making them into two?

128. `sharp'

NIHALI: d^{h}aar `sharp edge' (Marathi d$^h$aar); DENE-CAUCASIC: Basque zar-tzu; Caucasic: Abkhaz (Abzhai dialect) -tsʼar, ?Dagestan *ʌčʼʌ; ?Yeniseian *ʔete; AUSTRALIAN: Tiwi tɟ̈kini, tɟ̈wini; ANDAMAN: Önge giie care; AFRASIAN: ONGOTA čʼare `sharp'.

129. `skin'

NIHALI: tól; ?INDO-PACIFIC: Sepik-Ramu: Biksi tol `skin'; ANDAMAN: Pucikwar tailʌp, Juwoi t-lap-, Kol t-lɛp- `to skin'.

130. `sky (1)'

NIHALI: aggaso (Marathi aakaaš) `sky'; DENE-CAUCASIC: Yeniseian *ʔes `sky, god'; Na-Dene: Haida qwees, Tlingit kuts, Eyak koas `sky'.

131. `sky (2), above, up'

NIHALI: lege `up'; KUSUNDA: laga-i, lagai; LUZON NEGRITO: *lakup; ?AUSTRONESIAN: laŋit `sky'.

The proto form reconstructed from the languages spoken by the Negritos of Luzon is from Reid, 1994. This is one of a number of examples he proposes as possible retentions from pre-Austronesian languages. Note the lack of nasalization in all forms except the proto-Austronesian.

132. `small (1)'

NIHALI: othärä `dwarf'; KUSUNDA: dyoro `little'; ?AUSTRALIAN: *ɟulaŋ `small'.

133. `small (2)'

NIHALI: gits^{h}a `very small'; DENE-CAUCASIC: Basque (Souletin) guti `few, little, a little bit'; Caucasic *kotʼʌ; Burushaski k^{h}uuṭ `short'; Na-Dene: Eyak gutʼ `very small, tiny'; SUMERIAN: gud-da `short'; MUNDA: Mundari, Korku kaṭij, Santali kaṭiči `small, little'; INDO-PACIFIC: Andaman: Bea ketia, Bogajib ketawa; Tasman: [ME] kaita, kita; New Britain: Baining kitua `small'; AMERIND: *kʼutʼi `small, thin, narrow', *kuta `young'; AFRASIAN: [Illych-Svitych] *kʼ(w)t; DRAVIDIAN: *kuṭṭ-; KARTVELIAN: *kʼutʼ-; ALTAIC: Turkish küčük, Uighur kičik, Evenki köčaken `small'; Ryukyuan: kut-eng `to be small'; Inuit -kuči- `diminutive', Kamchadal kijg `fine, small';

SONGHAY: Gao kaṭu `few'; MANDE: Bambara suru, Mani kutũŋ ~ sutũŋ `short', suru `small', Mende kuto `short', kulo `small', Kpelle kutu, kùl̥ò `short'.

134. `snake'

NIHALI: kógo `snake'; AINU: okokko `snake'; NILO-SAHARAN: Berta (Sillok) kuke, East Sudanic: Kenuzi, Dongola; North Burun, Lotuxo kak, Didinga xukaa-t, kuka-t; Sila kookei, South Burun kaŋ `snake'.

135. `spider'

NIHALI: dzagali (malaay); AFRASIAN: ONGOTA tagara; AUSTRALIAN: Maung gar, Djingili garuǰi (?*ǰaguri) `spider'.

136. `to squeeze'

NIHALI: perto; AFRASIAN: ONGOTA birčʼas ~ pirčʼas `to squeeze'.

137. `to stand'

NIHALI: tsip ~ tsippo `to stay, stand'; SINO-TIBETAN: Garo, Moshang Naga, Kwoireng, Kachin tsaap, Namsang tsap, Naga-Bodo saap ~ saab ~ tsaap, Dhimal žap. Mikir ar-žap `to stand'.

138. `stick (noun)'

NIHALI: bedo; ANDAMAN: Pucikwar, Juwoi bɑat, Kol bɔ:t, Bale puutu, Bea putu-dʌ `stick'.

139. `stone (1)'

NIHALI: tsilaṭi (stationary) `lower half of grindstone'; AMERIND: Penutian *tola `stone'; KARTVELIAN: *ṭal- `flint'; ALTAIC: *t'iaal `shore, rock'; ANDAMAN: Bale taili, Bea tailii-dʌ, Cari mɛɔ tʌiyii; NIGER-CONGO: Temne (a) sar; Gurma (li)tan(le), Kasele (de)ta; Munga tei, Kam (a)tal, Mumuye tari, Bua, Gbaya ta, Sango te; Adele (de)ta, Akpafu ita, Gã tɛ, Kamuku tale, Bute seule, Bantu *-tade `stone'.

140. `stone (2), gravel'

NIHALI: paander tsunaa `gravel' (?< `limestone', if tsunaa `lime' is the mineral and not the fruit!); DRAVIDIAN: *panṭ- `rock, block of stone'; AUSTRALIAN: Parnkalla pand, punda, Caledon Bay panda, Nonga punda, East Mining bundoŋ, Meyu benṭa `stone'; BURUSHASKI: bun / bundó `boulder / pl.'

141. `stone (3)

NIHALI: kurup `stone'; MACRO-CAUCASIC: Hunza ɣooro; Basque harri `stone'; NILO-SAHARAN: Songhay: guru `hill'; Kanuri _gur ;testicle'; Maba kodo(k); Mimi (of Godefroy-Demombynes) kudu; Kenuzi, Dongola kul, Birgid kur, Temein kurɛt Merarit kira; Dinka kur, Lango got, North Burun guri-t `stone'; Bari kodu `hill'; NIGER-KORDOFANIAN: Malinke kulu `ball, rock, Susu kutu 'ball', Wolof nguli `testicle', xer `stone'; AUSTRALIAN: Kumbainggen kullam, Kauralgai, Saibalgal, Gumulgal kula, Prince Charles Bay kuula; NOSTRATIC: Dravidian *kal `stone', *kar(a) `bank, edge', Indo-European *ker- `cliff, stone', Afrasian *qr `cliff, mountain, hill'; AMERIND: *kela `stone'; AINU kuru `hill, mountain'.

142. `stone (4), mountain'

NIHALI: ḍoηgar; BURUSHASKI: Yasin ḍoon `hill', Hunza dan `stone, mountain'; SINO-TIBETAN: Old Chinese *troηʔ `hill, rock', Tibetan r-duη ,[9] Kachin ú-dùη `peak'; ? AUSTRALIAN: Dadidadi (Murray River, Victoria) <u>dh</u>aηga[9] `stone'.

143. `straight'

NIHALI: ḍhengi `erect; stand straight'; KADU: Krongo ɔrtɔη gwáán, Talasa (Tumtum) tüη-gɔ́:ná; NIGER-CONGO: Ewe, Twi tɛ̃:, Kpelle tɛ̃:, Vai teη `erect, straight'.

144. `summer'

NIHALI: agin; DENE-CAUCASIC: Caucasic *Ginʔʌ ~ Gin^{c}ʌ [Starostin, in Blazhek & Bengtson, 1995], Lak γwi, γwint-, Dargwa hani-š; Na-Dene: Haida q›in-at `summer'.

145. `tail'

NIHALI: pago; YENISEIAN: *puGadz `tail' [Starostin, in Ruhlen, 1994]

146. `tattoo, scar'

NIHALI: goday (d.Marathi god-ne) `tattoo; ANDAMAN: Bo kode, Kede kodo, Bale gade, Bea gadʌ, Juwoi kode, Pucikwar, Kol kodɛ `scar'.

147. `testicles'

NIHALI: gar gatál (Mundlay also records gaṭáál `penis' and garaa `inside of an egg ball, testicles); ANDAMAN: Cari kʌra-do `testicles'; AUSTRALIAN: Nungubuyu garaηa `scrotum'; ? SUMERIAN: gir; ? AFRASIAN: *k›wl; Ongota kirde `testicles'; URALIC: *koole; KARTVELIAN: *q›wer-; CAUCASIC: *q›waaq›wə(-<u>l</u>ə `seed, grain, egg', Budukh q›aluq `testicle, etc.'.

148. `this, that'

NIHALI: oṭi `that', dha `there', eṭe(y) ~ eta-re `he'; DENE-CAUCASIC: Dagestan *d$^h$ʌ `that'; Yeniseian *dʌ `he', *tu `this'; Haida dei `just that way', Tlingit de `now', Slave ti `this', Chiricahua díí `this thing'; Tibeto-Burman *day `this, that'; Old Chinese *tə `this'; AMERIND: *ta ~ ti `this, that, he'; NOSTRATIC: Afrasian *ta- `this (fem.), *dyə ~ dya `demonstrative stem'; Dravidian *taa `this'; Kartvelian *te-/ti- `this, that'; Indo-European *to-/te- `this, that'; Uralic *tä `this'; Altaic *t'ä- `that'; Ainu ta-p `this'; Chukchi-Kamchatkan *ti `here', Kamchadal tiʔ-n `this', Chukchi ȶ-lon `he', ʌt-ri `they'; Siberian Yupik ta-na; KHOISAN: Khakea [S5] te(a) ~ti(a) `this', ti `that'; /Nu-//›e [S6] ti `that', //Kh›au-//›e [N1] č̦i ~ tsi `this', č̦i `that', !Kung [N2] ši `that', !O-Kung

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[9] As in Australian usage, the symbol [<u>dh</u>] is used for the interdental laminated stop, the voiced mate of [<u>th</u>].

[N3] či `this';
? Hadza iča `there', šakase `that'.

149. `to throw (1)'

NIHALI: taar, taro; MUNDA: Ho ter; AUSTRONESIAN: Malay lantar; ANDAMAN: Aka-Cari ter cɑin `to throw'.

150. `to throw (2)'

NIHALI: tsenda; CHUKCHI-KAMCHATKAN: * čəŋtʌ `to throw'.

151. `today'

NIHALI: bay; KUSUNDA: ibe [Reinhard & Toba] `today'.

152. `tongue'

NIHALI: lãy, laiñ, laŋ [Konow]; KORDOFAN: Jomang d̪-uluŋɛ, Nding túlʌŋɛ, Ngile t̪-û'lû'ŋɛ, Tocho t̪-û'rʊŋɛ, El Amira liŋ, Tegem l͆-iáŋ-i, Rere T-ílíŋa; Katla liŋeT, Tima giliŋit; MUNDA: Santali, Mundari alaaŋ, Birhor aalaaŋ, Korwa alaŋg, Korku laaŋg, Kharia laŋg, Juang elaŋg `tongue'. Cf also INDO-EUROPEAN: Romance: Old Latin diŋgwa > Latin liŋgwa -> Italian liŋgwa, French laŋg, etc., < PIE *dnghw `tongue'.

153. `tooth (1)'

NIHALI: baru `to bite'; NOSTRATIC: Dravidian *palʌ; Korean iphal `tooth'; Altaic palʌ `molar'; ANDAMAN: Bogajib mɑ pɛla, Cari mɛɛr piilɛ , Kede miir piilɛ, Pucikwar pɛlä-dʌ, Juwoi pɛlɑk-lekile, Kol pɛlɑk-ce `tooth'.

154. `tooth (2)'

NIHALI: ménge `teeth'; AINU: *-imak; INDO-PACIFIC: [SWNG] Marind maŋgat, Jaqai maŋger, Auju Pisa, Jenimu, Aghu maga, Ashavi maŋya, Kukukuku menyi, Madinava maŋa; ?BURUSHASKI: -mɛ `tooth'.

155. `tree (1)'

NIHALI: daamboora `tree sp.'; DENE-CAUCASIC: Caucasic: Lezgia tam, Rutul dam `forest, grove', Lak ttama `wood'; Burushaski toom `tree'; Sino-Tibetan: Kukish *tum `log'; AMERIND: Macro-Tanoan *tamba `tree'; Equatorial *rampa `forest'; ESKIMO-ALEUT: *t'um(i)na `pine'.

Lest we forget, however, tropical and sub-tropical regions have hundreds of species of trees, so the likelihood of tree names repeating by chance is high.

156. `two'

NIHALI: ir, irar `two'; AUSTRALIAN: Tiwi yirara `two', irara, Yindjibarndi kula `pair', kura; INDO-PACIFIC: (Pacific Islands) Savosavo edo, Lavukaleve lelaol, Banua eri `two', Laumbe (kana)al, Reef lil `twenty'; KADU: *ɛ́:rá `two'; NILO-SAHARAN: Songhay (Timbukatu) kari `twin'; Lendu rro, Mangbutu adru, Moru ärr, irri, Madi eri, ri, Logo, Lugbara iri, Mangbetu rue, Tele rwio, Bongo riio; Dair, Gulfan ora, Dulman ore, Nera, Lotuxo arega, Merarit ware; Maasai are, Bari ɔri, Shilluk aryau, Dinka rou, Nuer rɔu `two'; ESKIMO-ALEUT: *ʔalə ~ γalɔ `two, second'; ?CHUKCHI-KAMCHATKAN: *kurxa `two, pair', *qulʔi `another, second'

157. `to urinate'

NIHALI: tsyo- ~ tsoo-; GILYAK: Amur č^{h}u `to urinate'; ?JAPANESE: šóoberi `urine'.

158. `vulva'

NIHALI: daana `clitoris'; BURUSHASKI: Hunza dʊη; AUSTRALIAN: Nungubuyu ni-Daη, Ungarinyin -adun, Margany Dinid `clitoris'; Larigiya dinda, Mbabaram bindun `vulva'; ?KHOISAN: Sandawe tsʼọηkʼo `vulva'.

159. `wear (on head)'

NIHALI: tshoη(koro) `masked (man); AUSTRIC: Miao-Yao *ntoη `to wear a hat'; Indonesian *tudaη `to cover, wear on head'.

160. `to weep (1)'

NIHALI: aapa `to weep'; AMERIND: Hokan *ʔiipia, Macro-Ge *poa `to cry'; KHOISAN: Hadza pipi; AFRASIAN: OMOTIC: Galila ef, Dime if; Gongan ep, Chara yef `to weep'.

161. `to weep (2)'

NIHALI: āto `tear (noun); AMERIND: *wuni; GILYAK wunt; ANDAMAN: Önge, Jarawa wana `to weep'; AFRASIAN: Cushitic: Yaaku anto, Tsamai ande, Gollango ^{c}ándʼe `water'; Chadic: Dangla-East àndè `water jug'; Nomotic: Diddesa Mao, Bambeshi Mao ʔántsʼɛ, ʔánsʼà `tear(s)(of eyes). These are the true cognates. There is also a Mao form [hande] `sweat' which is probably not the true cognate, despite appearances.

162. `wing'

NIHALI: paakho; MUNDA: Sora kappa, Korku kaapa; INDONESIAN *kapak `wing'.

163. `with'

NIHALI: b^{h}ai `with'; SINO-TIBETAN: Chaudangsi b$^h$aa `together', tee-b^{h}a ~ ti-b^{h}a `together with'; INDO-EUROPEAN: Sanskrit abhí; English bai `by', German bei `by'.

164. `woman, wife, widow'

NIHALI: b^{h}al(kol) `first wife (when alive); AUSTRIC: Indonesian *balu `widow(ed)'; ANDAMAN: Bale ab-pal, Bea ab-pʌil-dʌ `woman, wife'; INDO-PACIFIC: TASMAN: [NE,ME,SE] tibara(na) `woman'.

165. `wood'

NIHALI: dengara `huge log', ḍaaηo-ḍaaηo `branch': AUSTRALIAN: Malakmalak ɟeɳ; ANDAMAN: Önge daɳe, Jarawa taɳhi `wood'.

But see also the discussion of Kusunda and Önge below (pp.32-33). Perhaps `log' is the primary form and its application to `tongue', `bone', `horn', etc is an innovation in Önge and Kusunda.

166. `yesterday'

NIHALI: ts^{h}eesé (Korku ts^{h}o); BASQUE: atso; CAUCASIC: Abkaz yatsə Adyghe dəṞ-asaa; KOREAN: ʌje; KHOISAN: Sandawe ute ~ ʔute `yesterday'.

167. `young male sibling, child'

NIHALI: giʈa: `brother and sister, sibling, wife's younger siblings'; KUSUNDA: gitase [Hodgson] `child'; MUNDA: Santali giɖar, gaɖar `little children'; DENE-CAUCASIC: Caucasic *k$^{hw}$erc›i `youngster'; Na-Dene: Haida gyiit `child', giit `boy', Tlingit git›a `child', git `son', Eyak qeeč `child', qes `son'; Yeniseian *qət `children'; AMERIND: *kači `boy, child'.

In addition to the above, Nihali also participates in several of the global cognates proposed by Ruhlen and Bengtson. Mostly these give no clue as to where Nihali belongs in the global family tree.

The external relationships of Kusunda

Kusunda offers different problems from Nihali. The Kusunda lexicon appears much less infected by borrowings, but there is also much less of it. This deficiency is magnified by the uneven quality of the various sources. Whilst the differences are largely explicable, the information available remains far from satisfactory. One can only echo Professor Fleming's remarks concerning the apparent indifference of linguists to its demise. Let us at least ensure that in future no more languages like Kusunda are allowed to disappear with so little trace.

There are two extreme ways of looking at the different sources. One is to argue that Hodgson (from whose article the Linguistic Survey of India drew its Kusunda vocabulary) was a well-meaning Victorian amateur whose data are worthless, whereas those of Reinhard and Toba are the reliable findings of modern professionals. The other view is that Hodgson worked with a living language whose internal variation we can only guess at and recorded it faithfully by the standards of his age, whereas Reinhard and Toba worked with the aging and isolated survivors of a vanished language community whose imperfectly remembered idiolects may or may not have been representative of a language whose internal variation we can now only guess at. There is some truth in both views, but my own leaning is towards the latter. Certainly the limitations of our Kusunda data are such that we are in no position to pick and choose.

One cause of confusion is the variety of prefixes used in Kusunda. Hodgson frequently records forms in [č-], which means `my ("what is that?", "my eye"). Reinhard and Toba record the same forms with either [ta-] (this) or [g-] (its). Insofar as I have been able to detect a pattern, **ta-** appears to refer to body parts, particularly extremities, whereas **g-** refers to inanimate objects and non-extreme body parts.

There is some compounding of words. For instance, Reinhard and Toba give **t-au-dziŋ** `tongue' (my segmentation), where **t-** is `this', **-au-** is hole' and **dziŋ** is `tongue itself. This also appears in **ipi-gi-dziŋ** `horn', where **-ipi-** is `head', and **-gi-** is `hair'. One is immediately struck by the echoes of Önge, which has **daŋe** meaning all sorts of hard objects from `canoe' to `log' to `wood shavings' -- and also **ən-i-daŋe** `bone' and **ən-a-laŋ-daŋe** `tongue' (versus **ən-a-laŋ-e** `mouth'). Jarawa has **g-i-toŋe** `bone' (my segmentation). I would stop short of arguing that Önge **tabe** `head' is cognate with Kusunda **ta-ipi** `head' because the Önge word does not appear to take a prefix (unless this is a fossilized version of **ta-bi**). Nevertheless, there is the case of Önge **ən-i-bi-daŋe** `jawbone' (Jarawa **pitaŋ** `chin'); in such contexts **-bi-** usually means `arm' -- i.e. `its-arm-bone' -- but makes more sense here as an assimilated retention of **\*-(i)pi-** `head'. Note too that Hodgson records Kusunda **taü bi** `arm', while Reinhard and Toba have **tabi** `hand'.

The lexical evidence connecting Great Andamanese with Little Andamanese is very meagre. One of the handful of shared forms offered by Radcliffe-Brown is Önge **-iɟai** versus Bea **-ia ~ iya** `belonging to'. This is also recorded by Hodgson in Kusunda as a possessive marker. Note how Kusunda pronouns match those of Great Andaman:

	Kusunda	Kede	Pucikwar/Juwoi/Kol	Bo
Sg. 1	čhi ~ tsi	tui	tu-	tu-lʌ
2	nu	ŋui	ŋu-, ŋa	ŋu-lʌ
3	git, gidanun	kitɛ ♂		olʌ ♂
		kudɛ ♀		kitɛ ♀
Possessives				
Sg. 1	či-yi (Hodgson)		tiiye	ti-e
2	ni-yi (Hodgson)		ŋii-ye	liʌ dɛle♀
PL. 1				tii-ye
2				ŋiie
3				iiye

Ruhlen (personal communication) points out that there is a shared alternation between **-u-** nominative and **-i-** possessive, and adds that same alternation is also found in the Indo-Pacific languages of Halmahera.
It would be reckless to assign Kusunda to Andamanese simply on the basis of the above. Apart from the present lack of an adequate sub-grouping for Indo-Pacific, there is also the wider question of Macro-Australic to consider. Here is another piece of evidence linking the two Andamanese families:

LITTLE ANDAMAN: Önge unya-gii-le `man'; GREAT ANDAMAN: Pucikwar nu-le, Juwoi ne, Kol nu `person', Bale nu-le, Cari kiiditɑn ʌinyu `people' (cf Kusunda gidənun [Reinhard & Toba] `person, he').

Some of the above may be familiar! For instance:

KUSUNDA: ñu [Reinhard & Toba] `man', niyu [Reinhard 1976] `person'; AINU: ainu, niyu `person'.

However, I should also point out that the form has a wider distribution:

INDO-PACIFIC: [Timor-Alor-Pantar] Makasai anu, Bunak ɛn `man'; NA-DENE: Tlingit na `tribe, people', Athabaskan *-ne- ~ -n `people, person'; SUMERIAN: na `person', ni-ta, ni-tax `man'; INDO-EUROPEAN: *ner- `man, male'; ALTAIC: *niarʌ `man, person'; KADU: Miri ñurí, Talla (Kadugli) ñɔ:rí, Tolibi (Katcha) ñorí, Krongo ñaarí, Talasa (Tumtum) ñʊrí `husband'; NIGER-CONGO: Mande nu, Kpelle nũ `man', Bambara nyi `partisan de...', Nalu nyie, Bulom nɔ: `man', Mandyak (ba)nya `men'; Mossi ni, Dagomba niri, Kasele onyi; Adele (e)ni, Yoruba ni, Likpe nii, Abua ɔni, Nyidu unʌ, Koro (i)nye; Daka ne, Kam nyi, Onguda nyi(re) `man'.

Nevertheless, if the above example does not conclusively demon-strate Macro-Australic, it does contain additional pointers. Önge **unya-gi-le** `man' contrasts with **unya-ɑ-le** `wife', while Hodgson's Kusunda data include many forms including **-gya** `male'. One also finds Djingili **ŋambi(-)gua** `husband' versus **ŋambi(-)lini** `wife', Warndarang **njal(-) guyi**. I do not yet have sufficient Australian data to say whether this is even more widely spread there. Finally, Ainu **okkai** `male' may also belong in this set.

I must of course stress that these misgivings as to the true membership and internal structure of Indo-Pacific should not be interpreted as an attack on Greenberg so much as an attempt to refine his conclus-ions in the light of data that were not available to him during the 1960s. Indeed, Greenberg himself had reservations about whether the Little Andaman family even belonged in Indo-Pacific, let alone with Great Andamanese, and described the exclusion of Australian from Indo-Pacific as `tentative'. As for Nihali and Kusunda, Greenberg attempted to classify Nihali in 1953 using only Grierson's data, and was unable to place it anywhere. As for Kusunda at the time, it was buried in a mass of poorly described Himalayan languages which even the Tibeto-Burman specialists had not yet sifted through.

In the continuing absence of adequate data for Little Andamanese, these reservations remain as serious today as they were in 1971. The late nineteenth century manuals give very little Önge, while the only wordlist I have been able to find for Jarawa is the one collected by Lt. Colebrooke in 1790! As for the Sentinelese, their language has been assigned to Little Andamanese on the basis of no evidence **at all**. It would serve us all right if, when they finally talk to us, their speech turned out to be Uralic! The one consolation is that the same reticence that denies us linguistic data may at the same time keep their language alive. Linguists should nevertheless be careful to ensure that these precious languages do not go the way of Kusunda.

What follows are all the correspondences of sound and meaning I have been able to find (so far) connecting Kusunda to other lang-uage families. Those including Nihali were presented earlier, and are not reproduced here. They are numbers 15 `bitter (2)', 27 `child (3)', 33 `cloud', 57 `fire (2), 83 'interrogative', 89 `to live', 96 `moon, 107 `one (2)', 118 `root (3), 131 `sky (2)', 132 `small (1)', 151 `today', 167 `young'.

168. `to be above'

KUSUNDA: drasu ok `above' [Hodgson]; ELAMO-DRAVIDIAN: *uk- `to be above'; AFRASIAN: Somotic: Dime oku `peak, teat, nipple', ok-se `over'.

169. bark, skin'

KUSUNDA: gitan ~ gitɨ `skin, bark'; INDO-PACIFIC: Tasman [NE] kite (-na) `skin'; Andaman: Bogajib kait, Bale, Pucikwar, Kol kaic; [BO] Telei katua; [SWNG] Kandam, Lower Muju, Marapka, Niinati, Metomka kat, Kaeti, Mandobo kota, Wambon, Dumut kotae; [CNG] Ekari kado; [WNG] Tauata kotsi(pe), Goilala koti(pi); Tanah Merah (1) katane `skin'; AUSTRIC: Miao taw; Mon-Khmer: Muong ta, Rongao kdoh, Sedang kũ̈too, Katu toʔ, Bahnar kadoʔ `bark'; INDO-EUROPEAN: *kuuti- `skin', *(s)keu-`to cover'; Latin kut-is `skin'; URALIC: Finnish kesi, kete `skin, peel', Saami gɨt, gɔtt `hide'; NIGER-CONGO: Limba koto `skin'; KADU: Krongo ndigoṭo, koṭo `bark'; ?AMERIND: Ojibwe -škatay `skin, hide'; Chipaya skiši `skin'.

170. belly'

KUSUNDA: tamat, imaat; AMERIND: *mat; TASMAN: [W,SE] lomati(na).

171. `below, underneath'

KUSUNDA: tumai [Hodgson] `below'; INDO-PACIFIC: [Halmahera] Tobelo timi `underneath'; [NNG] Kwesten tum; [SNG] toma `earth'; [CNG] Gende teme, Fasu tomo, Sene dome `underneath'.

See also No.185 `earth'. Greenberg put the above Indo-Pacific together. Possibly Kusunda **tumai** `below' has the same relationship to Kusunda **doma** `earth', **dum** `dust'.

172. `bite, chew, tooth'

KUSUNDA: kam-dzi `it bites'; INDO-PACIFIC: [SWNG] Pisirami, Tagota, Lake Murray, Babwa kama `tooth'; SINO-TIBETAN: gəm `molar'; AMERIND: Almosan-Keresouian *k›am `to bite'; AFRASIAN: *k›əm ~ k›am `chew, bite, crush, cut to pieces'; INDO-EUROPEAN: *k›em-b(h)- `chew' [Bomhard], *gembh- [Watkins, Pokorny] `tooth, nail', English kom `comb', Russian zub `tooth'; CHUKCHI-KAMCHATKAN: *kwəp-nə `tooth'; JAPANESE: kamu `to bite'; AINU: ¿amse; NILO-SAHARAN: Songhay: kaama `chew' (Afrasian loan word?).

173. `black'

KUSUNDA: pasidai [Reinhard & Toba]; AINU: pas `black'

174. `blood, red'

KUSUNDA: uyu `blood'; KADU: Tolibi (Katcha) uṭ̈ṭɔ́yɔ́, Sangali (Tumma) ʌtɔ́yɔ́ , Krongo ʌyɔ:ni `red'.

175. `brother, sibling, uncle'

KUSUNDA: mam `older brother; AUSTROASIATIC: Kharia mamu `mother's brother'; NIHALI maam-a/-i `MoBr, MoBrWi' [Bhattacharya], maamaa `MoBr', maawsi `MoSi' [Mundlay] but maai `Mo'; INDO-EUROPEAN: Indic: Marathi, Hindi mama `MoBr'; AFRASIAN: Cushitic: Dahalo ʔáama `MoBr'; Semitic: Arabic ^{c}am `FaBr'.

Many examples of the famous MAMA root for `mother' occur world wide but this `uncle' term is more limited. While it may be built up from a base in MAMA, it does not reduce to a nursery word, being a formal kinship term used for a male kinsman.

176. `bush'

KUSUNDA: watsi; NA-DENE: Tlingit wásʼ `bush'; ?CAUCASIC: *wïïtsVV `grass'; Chechen buts; Kabardian wədz, etc.

177. `cold, ice'

KUSUNDA: khaŋu [Reinhard & Toba], k^{h}aŋ go [Hodgson] `cold'; YENISEIAN: Ket quuŋ-el, quu, qou `ice'; AUSTRIC: Indonesian *qənaw; ESKIMO-ALEUT *qenʔa `cold', qanʔe `snow'.

178. `come, go'

KUSUNDA: da [Hodgson], tsaʔ-nə [Reinhard & Toba]; TASMAN: [NE] tagana, [ME] takana, [SE] takera, [N] takeri `go'; AMERIND: Chibchan-Paezan *ta, *taka `come', Hokan *ta `go out', Penutian tok `return'; ESKIMO-ALEUT: *taɣə `come'.

179. `day (unit)'

KUSUNDA: uŋutsa; ?BURUSHASKI gʊnts `day (unit)'.

180. `deer'

KUSUNDA: qoyo `red deer'; SINO-TIBETAN: *d-kiy `barking deer'; YENISEIAN: Kott koodza, Pumpokol xadzu `elk'.

181. `dirty'

KUSUNDA: huwə `dirty'; KHOISAN: Sandawe hweesi `to be dirty'; AUSTRALIAN: Tiwi: kuwuliŋini `dirty'.

182. `dog'

KUSUNDA: agə; KHOISAN: Sandawe gaga; SUMERIAN: gi-tur; KOREAN gɛ `dog'.

183. `dry'

KUSUNDA: qaaiwan; JAPANESE: kawaita `dry'; AUSTRIC: Indonesian *paɣaw ~ pəɣaw `hoarse'; ?AMERIND: Andean *paki `dry' (all < *pakaw).

184. `dust, cloud, smoke'

KUSUNDA: duŋi `smoke'; SUMERIAN: dungu `cloud'; KHOISAN: Sandawe tlʼu ŋ gu; AFRASIAN: South Cushitic: Iraqw tlʼaŋgu, Kwadza tlʼuŋguwa `cloud' (? loan word from Khoisan); KADU: Talasa (Tumtum) Tuŋgu, Miri nduŋuru, Tolibi (Katcha) `nDúŋgúru `dust'; AUSTRALIAN: Yindjibarndi thuŋka `sand', (Victorian) Dadidadi dun, Yodayoda doŋa `smoke' (and perhaps also Tiwi tiŋati `sand'?)

I suspect the primary meaning here is `dust', as retained in Kadu `dust' and Yindjibarndi `sand'. This shifted in Sumerian and Sandawe to mean `cloud', presumably from `cloud of dust', presumably Kusunda `smoke', from `cloud of smoke', is a further development of this.

185. `earth'

KUSUNDA: doma [Hodgson] `earth', dum [Reinhard & Toba] `dust'; AMERIND: *tampi; INDO-PACIFIC: [NNG] Kwesten dum, [SNG] Dibin toma `earth'

But see also No. 171 `below, underneath'; Incidentally, Greenberg's AMERIND *tampi includes [Almosan-Keresouian] Central Algonquian *athaam `under', Cree taami `underneath'; Salish *timix$^w$ `under', Shuswap temʔ `bottom'.

186. `eat'

KUSUNDA: am [Hodgson] `eat'; no-ʔam-nə `thou eatest' [Reinhard & Toba], ta-ama-nan `I eat' [Reinhard 1976]; NILO-SAHARAN: Fur -am; AMERIND: *am ~ ama; SINO-TIBETAN: *ʔɪmH `drink'; AFRASIAN: proto-Nomotic probably was [*m-ʔ], while [am] occurs in Cushitic. Another Afrasian cognate *T^cm- `to taste' often is mistaken for that root.

187. `egg'

KUSUNDA: goa, gwa `egg'; ANDAMAN: Önge gwaganɛ `turtle egg'; NA-DENE: Haida qáw `bird egg'; SINO-TIBETAN: proto-S-T *Qo(w)H `egg'; Tibetan s-go-ŋa `eggs, spawn'; Burmese uʔ `egg'.

188. `eye, see'

KUSUNDA: č-iniŋ [Hodgson], ta-inin [Reinhard & Toba], -iniŋ [Reinhard 1976]; NIGER-CONGO: Bulom (Mampa) nyeny, Bidyogo (Ankaras) ne; Malinke, Dan nya, Soso nia; Nafana nye(ne), Sisala ni, Mossi ni(fu); Ahlo (e)nu, Ibo anya, Abure enyi, Efik enyin; Yungur nu, Longuda nyu(la), Kam anu, Jen niŋ, Kumba, Vere nɔɔ(r), Nielim nyi; NILO-SAHARAN: Fur nú `eye'; East Sudanic: Nubian (Mahas) nini `eyeball, ball of eye', Dinka nyiŋ `eye'; Kunama ni-ti `to see'; AUSTRALIAN: *na `to see, look at'; INDO-PACIFIC: [TA] Oirata nana, ina `eye'; Makasai ena; [WNG] Waipu no `eye', Jahadian nu, ni `see'; [NNG] Anaberg na `to see'; Nimboran, Kuangsu nu `eye'; [CNG] Mono ini, Foe qana, Agarabi one, Mogobineng ona; [NENG] Banara na `to see; [ENG] Mailu, Domara ini 'eye', Nemea nii `to see', Suki, Agi, Hagari, Sogeri, Maian, Koita ni, Minjori, Suambe niie `eye', Koiari `eye, face'; [UNG] Tate ini `eye'; BURUSHIC: Hunza **nəa** `ball of eye', Yasin **níni** `pupil of the eye'; CAUCASIC: West: Pakhy na, Abzax ne ~ nne, Circassian ne ~ nne `eye'; AFRASIAN: Berber inni; Egyptian nw `to see'; Semitic: Arabic ʔinna, Hebrew hinn- `behold!', Modern South Arabian: Mehri náwnəw ~ noonəw `pupil of the eye', Jibbali nónú `pupil of the eye; baby ; Chadic: Angas nai, Jeng (Bata) naan, Tera na, Jara nana `to see'; Somotic: Dime nòno `mote, sand of eye, `sleepers''; DRAVIDIAN: *naaṭ- `seek, gaze, look at'; Amerind: *neu `to see'; GILYAK: Amur ñu `to look'; KOREAN: nun `eye'; AINU: nu, nu-pe `tears' (`eye' + `water'); KHOISAN: /Kham [S1], //Ng-!ˀe [S2] /ni, //Khegwi [S3], /Nu-//ˀe [S6] /nee; Khakhea [S5}, !O-!Kung [N3] /ne `to see'.

It is particularly difficult to say exactly what original non-click consonant (if any) the nasal release here corresponds to. My own guess would be ***ŋit ~ ŋin**. If the `Khoisan problem' is ever to be cracked, it will be etymologies like this which point the way.

189. `fat'

KUSUNDA: biji [Hodgson], bidzi [Reinhard & Toba] `fat (adj.)'; BURUSHASKI: Hunza biis, Yasin bɛs `fat (noun)'; CAUCASIC: Botlix bišši, Cham bess-ab, Hunzib boš-eru `greasy, fat'; ANDAMAN: Bale bod-, Bea patʌ- `fat(adj.)'.

190. `flower'

KUSUNDA: gipoan [Hodgson], geipəŋən [Reinhard & Toba]; AUSTRIC: Green Miao paŋ, Iu Mien pyaaŋ, Laqua puŋ, Austronesian *bunga, Muong pong, Vietnamese bɔng hoa, Katu ʔbowng `flower'.

191. `fowl, bird'

KUSUNDA: dəma `chicken'; CAUCASIC: \*tˀimHʌ; YENISEIAN: duma `bird'.

Although, as with trees, there are hundreds of tropical bird species too.

192. `fruit'

KUSUNDA: yegiyan [Hodgson]; LUZON NEGRITO: [Reid, 1994] N.Alta ian, iyan `fruit'.

193. `full'

KUSUNDA: qomba full'; KHOISAN: /Nu-//ˀe [S6] !gum; !Kung [N2] !kum; ?Sandawe ≠oome `to fill'.

194. `to give'

KUSUNDA: ai [Hodgson] `give', ia-tənan `I give' [Reinhard 1976], yawu `give (imperative)' [Reinhard & Toba]; INDO-PACIFIC: [SNG] Jibu, Makleu ai, Riantana ii, Kimaghama iye; AMERIND: Macro-Carib \*i; KHOISAN: //Khˀau-//ˀe [N1], Naron [C2] auu `to give'.

195. `grass'

KUSUNDA: gadzi; AFRASIAN: Ongota: xaaše; CHUKCHI-KAMCHATKAN: *viχadza `grass' (NB: Gilyak vaχ `moss').

196. `hair'

KUSUNDA: gyai-i [Hodgson], ipi-gi [Reinhard] (ipi `head'); BURUSHASKI: γoy-aŋ ~ γuy-aŋ; NA-DENE: Tlingit χaw, Athabaskan \*Xaʔ `hair', Eyak -χu `fur, body hair'.

197. `head'

KUSUNDA: č-ipi [Hodgson], ta-ipiy [Reinhard & Toba], ipi [Reinhard 1976]; ANDAMAN: Önge tabɛ, Jarawa tabe; ?AINU: pa; ??NILO-SAHARAN: Fur tabu, tebu `head'.

See discussion on pages 32-33 for possible segmentation.

198. `hear'

KUSUNDA: maŋʔbo [Hodgson] `hear', maauŋbatan [Reinhard 1976] `I hear'; MON-KHMER: Muong maŋ, Sedang hmaŋ, Bahnar mũŋ, paŋ, Bru tõmũŋ, kamaŋ; AUSTRALIAN: Tiwi: muŋuma; NILO-SAHARAN: Songhay: ma; NIGER-CONGO: Mande: Bambara mɛ̃ ~ mê, Kpelle mal̥ ~ mɛni, Mende mɛni, Dyula, Maninke mɛ, Soso mɛ ~ mɛγi `hear'.

199. `hoe'

KUSUNDA: šinka; NILO-SAHARAN: Fur suŋgop `hoe'.

See Nihali No.41 `dig, chop'; the root form appears to be *suŋ-kop.

200. `honey (1), bee, sweet'

KUSUNDA: toi `honey'; AINU: ka-soyai `bee'; AFRASIAN: Ongota šookaya `bee, honey'; SHABO: soy ~ šoy `bee'; KHOISAN: Hadza t›õy `sweet'; Sandawe t›wai-i `sweet'; //Kh›au-//›e [N1] žo, !Kung [N2], !›O-!Kung [N3] jo `honey, bee';

201. 'honey (2), sweet'

KUSUNDA: gitəŋ; ANDAMAN: Önge taŋəi; KHOISAN: Tati [C1] denee, Naron [C2] daniša, Nama [C3] dán-i-b, !Ora (Korana)[C] dani-s `honey', //Ng-!›e [S2] t›yaŋ, !Kung [N2] táŋ 'sweet'; AFRASIAN: SOUTH CUSHITIC: (loan word?) Iraqw danu, Alagwa, Burunge dinu `honey'; AUSTRALIAN: Djapu (d.Yuulngu) ŋutaŋ `bee?, Gumbaynggir duŋaar `edible bee, honey', Wargamay ɟaŋal `honey' (Pama-Nyungan *d- > ɟ-)[10], Margany d̲indiñ `bee, honey', Uradhi añɟin `bee, honey'

202. 'house'

KUSUNDA: bahi [Hodgson] `house'; INDO-PACIFIC: [SNG] Hiwi aba, Foraba be, Sesa pei `village', Ibukairi be, Jelmek ebi, Teri-Kawaisch paya `house'; NIGER-CONGO: Tschi o-fi, o-fi-é `home compound', Gbati a-pi `compound, home', Mussu pi `home, house'; Koro e-pfi `house', Akwa e-fi `yard', Nyangbo ke-pi `home, house', Bamana pu-γe `house', Senufo pi-ge `dwelling'; AMERIND: (Macro-Ge) Bororbai baa `house', baha `village', Chiquito poo, Masacra pa `house'.

203. 'how many?'

KUSUNDA: asina `how much?'; AFRASIAN: Ongota ašana kuyda `how many?'.

204. `husband; male relative'.

A symbol, not shown here, but roughly [j] with a line through it, representing a voiced palatal plosive in the IPA, was used in the original. That was not changed in the main text.

KUSUNDA: duwoi [Hodgson], duwəi [Reinhard, & Toba]; NILO-SAHARAN: Fur duuo `husband'; SINO-TIBETAN: *do 'to be related by birth or marriage'; AUSTRALIAN: Ngalkbon duei `husband'.

205. `in, inside'.

KUSUNDA: gamada [Reinhard & Toba] `in'; INDO-PACIFIC: [TA] Oirata mudu `inside', Makasai -mutu `in'.

206. `like, love, want'

KUSUNDA: tə-mənətə `I like', mɛnau-nə `I want' (with apparent root -mənə); KHOISAN: Sandawe meena; NILO-SAHARAN: Kunama me `to love'; NIGER-CONGO: West Atlantic: Temne manɛ `lover', Diola mane `to love', Gola mana `to wish, love', o-mɔn `friend'.

207. 'liver'

KUSUNDA: kammu, qaamu `liver'; CAUCASIC: Andi k̄ʼunu, Bezhta, Hunzib koma `kidney'; NILO-SAHARAN: Songhay: Djerma hamɛ; INDO-PACIFIC: [NENG] Sihan kɛma-, Bau kema-, Silopi kemu-, Gumalu, Panim gɛmʌ-, Amele gema-, Bagupi gɛmɛ-, Utu gɛmu-, Rempi gʌma-, Samosa gʌmɛ-, Murupi gʌm-, Wamas, Garus gamɛ-, Mosimo, Gal gami-, Garuh gama-, Saruka, Nake gam-, Dimir, Malas gaman; Pay kamɛni, Ulingan, Moere kɛma, Abasakur kama$_{\tilde{n}}$i$_{\tilde{n}}$i; Sinsauru gʌma, Asas kamɛ-, Sausi kamo, Usino gʌmʌ, Sumau kʌma, Urigina gama, Danaru gamʌm, Kwato kamamu `liver'.

208. `long (1)'

KUSUNDA: hwa$_{\eta}$ gai [Hodgson]; ANDAMAN: Önge oi-ɟagai `long'.

209. `long (2)'

KUSUNDA: sara [Reinhard & Toba] `long'; SUMERIAN: sir `to be long'.

210. `louse'

KUSUNDA: kiʔ [Reinhard 1976]; BURUSHASKI: khəru; INDO-PACIFIC: [UNG] Warenbori ki-ro; AINU: ki, uruki; AMERIND: *ikʼe; AFRASIAN; Omotic: Mao kʼiše, Dime kʼəs, Bako, Hamar kʼasa `louse'.

211. `many'

KUSUNDA: manyi; TASMAN: [W] muunera, [SE] muunalane; AMERIND: *moni `many'; SINO-TIBETAN: ma$_{\eta}$ `big, older'; INDO-EUROPEAN: Germanic *manag-az; ALTAIC: *maanʌ; URALIC: *mone- ~ mune; KOREAN: man-in; JAPANESE: mansi- `many'.

212. `monkey'

KUSUNDA: ugu [Hodgson]; SINO-TIBETAN: *(g)woy; SUMERIAN: ugubi `monkey'.

213. `mouth'

KUSUNDA: ta-uta [Reinhard & Toba]; AUSTRALIAN: *dawaŋ `mouth', *da `eat'; NIGER-CONGO: Gã ḍa; Bambara, Dyula, Maninke, Mende la, Vai da, Kpelle ḍa; Soso, Mwi dɛ, Boko ḍɛ́:, lɛ́:; NILO-SAHARAN: Fur udo; Koma (Madin) ita, Kwama t›wa, Gule ite-n, Uduk twaʔ; Kunama uuda, Ilit wida; Sillok utu, Tornasi udu; Mangbutu uti, Moru ti `mouth'.

214. `name'

KUSUNDA: giji [Hodgson], gidzi [Reinhard 1976]; NILO-SAHARAN: Kunama kiida, Ilit u-kude `name'.

215. `new'

KUSUNDA: dzinəi [Reinhard & Toba], dziŋdai, dzinaʔi [Reinhard 1976]; KHOISAN: Hadza dzana; ?SHABO: čo ~ tso `new'. Colleague Bengtson also suggests three Altaic forms, based on Starostin's reconstructions, to wit, proto-Korean *sái < proto-Altaic *za(i)ŋi; proto-Turkic *jaŋï ~ jeŋi; proto-Mongolian *sini.

216. `place'

KUSUNDA: tawə `cleared space', (hun)təwu `(far)place'; ANDAMAN: Pucikwar ar-tiwe-dʌ `place', tiwe-l-ar-wiike-dʌ `far', Juwoi ra-tiiwe-lekiile `place', tiiwe-t-ra-wiike-lekiile `far' Kol a-tiiwe-ce `place', tiwe-ta-wiike-ce `far'.
Note that not only do both have the same word for `place', but they also both use it in the construction `far'.

217. `poison'

KUSUNDA: qaʔ [Reinhard & Toba]; ANDAMAN: Juwoi ak-, Kol -ɛʌk- `poison'.

218. `red'

KUSUNDA: ban uba [Hodgson], bauŋaŋ [Reinhard & Toba], baauŋ [Reinhard 1976]; MUNDA: Sora jabaŋ; AUSTRONESIAN: Javanese abaŋ `red'.

219. `rotten'

KUSUNDA: qawandân `rotten'; AFRASIAN: *k›wəy ~ k›way; INDO-EUROPEAN: *k›wey ~ k›woy `to fester, be putrid, foul, purulent (Bomhard); ?DRAVIDIAN: *kii- `pus, putrid matter'.

Although **qawa(n)-** appears cognate with the other forms, what **-dən** means remains a mystery. Unfortunately, Reinhard & Toba's is the only form given for `rotten'; there are no other versions to compare it with. Elsewhere the suffix appears only in **pasiydən** `blue' and **qâsâdən** `one'. In the probable absence of further Kusunda data this mystery looks set to last. [Editor's Note: The supplementary evidence of [qawəndən] `old' seems to justify the presence of an adjective formant [-dən].]

220. `sand'

KUSUNDA: gəli, gali; AUSTRALIAN: Ngarndi galaŋa `sand', Margany galburu `sandhill', Gunya gaḍila ~ gaḍiya, Wembawemba gurəg, Madimadi guragi `sand'.

221. `see, search'

KUSUNDA: tsandzi [Reinhard & Toba], tsaaŋanan [Reinhard 1976] 'see'; AUSTRIC: Tai *ʔdu `look at', Kam-Sui *do `see', Indonesian *tinaw ~ tindlaw `look at closely'; AMERIND: Penutian *tʼen `to look', Chibchan-Paezan *tene `see', Andean *teli `eye'; ELAMO-DRAVIDIAN: *tsu-, KARTVELIAN: *tsaw-, ESKIMO-ALEUT: *ts(u)- `see'; NIGER-CONGO: West Atlantic: Temne ṭɛŋ-s `to search', tana `to see'.

222. `sharp, cut'

KUSUNDA: gɚə̣i; AUSTRALIAN: Nungubuyu maɭ̥gura, Malakmalak kɛrkɛr `sharp', Alawa kurp `to cut'; AFRASIAN *qwr `cut, dig, opening'; DRAVIDIAN: *kuur `sharp'; KARTVELIAN: *qwer- `to castrate'; URALIC: *kurʌ `knife'; ALTAIC: *kur-č̆a `sharp'; JAPANESE: kiru `to cut'; SUMERIAN: kur `cut, separate, divide'' ?BURUSHASKI: Hunza hiir-ʊm, Yasin hɛrɛš-ʊm `sharp'.

223. `to speak, say'

KUSUNDA: pwaktoba [Hodgson] `speak'; INDO-PACIFIC: [SWNG] Jaqai bak, Telefol bokoo `say'; ?AFRASIAN: *bə̣h- ~ bah-; ?INDO-EUROPEAN: *bah- ~ beh- `speak, say'.

224. `to stand'

KUSUNDA: doŋtsoʔ-anan `I stand'; AUSTRO-TAI: *zeŋzeŋ; MON-KHMER: Vietnamese dũŋ, Muong twãŋ, Bru tayũŋ; NILO-SAHARAN: Songhay: Djerma tun; KADU: Miri ɟiŋɛ́, Tolibi (Katcha) ɛɟiŋɛ́, Sangali (Tumma) aɟiŋ̃ɛ́ `to stand'.

225. `to take'

KUSUNDA: ma; AUSTRALIAN: *ma, Tiwi maruṟi; INDO-PACIFIC: [ENG] Tauata mi, Kotoi ma `to take', Namau miai `to take away'.

See also No.74 `hand, give'.

226. `thunder, lightning'

KUSUNDA: kʰila; ANDAMAN: Bale kuruudʌ-ke, Jeru kuruḍe `thunder'; AUSTRIC: Indonesian *kilat `lightning', *kilap `glitter'.

227. `tomorrow'

KUSUNDA: gorak ~ goraq; AUSTRALIAN: Nungubuyu agaṟay `tomorrow'.

228. `vomit'

KUSUNDA: əd-ŋəŋ `I vomit', waq `vomit (noun)'; ANDAMAN: Bea ad-we, Bale ʌd-wakia `to vomit'; AUSTRO-TAI [Benedict] *ɭ̥u(w)aq `vomit'; AUSTRALIAN: Wulna wagqua `vomiting'.

In the Andaman examples **-wakia** is presumably a noun meaning `vomit', as is the Kusunda **waq**. In the whole of Andamanese the only other instances of **ad-** are in Bale, Bea **ad-gin** `bear a child', **ad-lee** `to fight' and Bale **ad-at**, Bea **ad-eti** `to be born'. From this I would deduce that **ad-** is a verb meaning `to throw out' or `to throw up'. The two elements of the Andamanese compound **ad-wakia** are preserved separately in Kusunda as the verb `to vomit' and the noun `vomit' respectively. The supposition that Kusunda əd-ənə `I vomit' was originally `I throw (up)' is reinforced by the Burushaski ət- `to throw'.

229. `to walk'

KUSUNDA: aban [Hodgson] `walk, move'; ANDAMAN: buni-ɟawa; AINU: urepuni `walk'.

230. `water'

KUSUNDA: taŋ `water', dzandzi [Reinhard & Toba] `large river'; INDO-PACIFIC: [UNG] Warenbori dando (?dan-do) `water'; NA-DENE: Haida taŋ `sea water', Eyak tą̃h `waves', Galice ta- `water (in compounds); Chipewyan tà-, Navajo tá- `water'.

231. `wood'

KUSUNDA: gigidzi; SUMERIAN: giš; YENISEIAN: *ʔokse; AMERIND: *ake `wood'.

232. `woods, forest'

KUSUNDA: giloŋ; ?SINO-TIBETAN: *b-liŋ `forest'; MON-KHMER: Vietnamese, Muong rũŋ, Mnong Gar kroŋ, Katu krõŋ, Sedang koŋ `woods'.

233. `year, old'

KUSUNDA: dzinoʔi; BURUSHASKI: Hunza den, Yasin deen `year'; BASQUE: a-din `age', a-din-tsu `old'; ?CAUCASIC: Nakh *dee(n) `day'.

Historical Inferences

To my mind the only model that accounts for both Andamanese and Tasmanian membership of Indo-Pacific is one in which Proto-Indo-Pacific was the first language to enter SE Asia, where it split into a number of descendant families, one of which was the first language to enter Australia. It was followed into SE Asia by Proto-Australian, whose descendants displaced Indo-Pacific across much of Sundaland and Australia. The recent expansion of Pama-Nyungan would have obliterated the last Indo-Pacific languages on the Australian mainland, and might well have done the same to Tasmanian but for the rising sea level. Geoff O'Grady has argued that this Pama-Nyungan expansion was a result of contact with Austronesian culture. This contact would not necessarily have to have taken place on the Australian mainland. According to my model, the closest relatives of Australian will have been on the islands to the west -- now replaced by Austronesian. Unless the subgrouping of Pama-Nyungan (when finally arrived at) suggests an inland locus of dispersal, it remains possible that Proto-Pama-Nyungan originated outside Australia and may even have been carried in across the Timor Strait. Certainly, the ability of Pama-Nyungan speakers to supplant their predecessors across four-fifths of Australia may seem more explicable as an intrusion from overseas than as an expansion from within.

What must always be kept in mind is that when the ice sheets melted, huge tracts of SE Asia disappeared under sea water. This will have forced an equally huge number of different language communities to take to their boats. In the ensuing scramble, first for survival and then for a new home (or to avoid being driven from one's old home), any one could have ended up anywhere. This would account for the extreme linguistic diversity of both New Guinea and Northern Australia, where languages from a much wider area have found refuge.If it were not for Austronesian, we might find that the closest relatives of some of the Australian and Papuan subgroups were even to be found west of the Wallace Line.

My point is that such a model requires Indo-Pacific to have begun to fragment somewhere near modern Malaya, and Macro-Australic to have fragmented even further north and west -- perhaps even in India itself. The great distance separating Nihali and Kusunda supports this, while the model also allows for the particular relationship between Nihali and Ainu. It is difficult to account for the dispersal pattern implied by an Out-of-India model except as a result of the original Human migration into the area. This has obvious implications for the ages of the relationships concerned -- but all this awaits a more secure sub-grouping of Macro-Australic and its constituent phyla.

Such a migration would be no more than the continuation of the original Out-of-Africa movement. My research so far suggests that the Human language family tree will also have an African locus of dispersal. It remains to be seen how this enlarged Macro-Australic aligns with its neighbors to the west.

While I was at it . . .

It has been argued that the global cognates proposed by Ruhlen and Bengtson et al are chance resemblances that anyone could find if they sifted through enough data. Ruhlen pointed out that if this is so, it should be possible to match his evidence for TIK `one, finger' and PAL `two' with a similar number of forms for PAL meaning `one, finger' and TIK `two'. In the last three years I have failed to find a single example of either TIK `two' or PAL `one, finger', though I have looked everywhere.

Sources cited in the text

This is not, of course, a list of every source I have consulted. There simply would not be sufficient space for me to list all of those. In addition to the following, I have also made great use of materials provided for me by Messrs Bengtson and Fleming, whose advice and assistance at all stages have been invaluable. Thanks also to Professor Thilo Schadeberg for letting me have a copy of his wordlists for the Kadu languages. Sir, the world still needs your lists for Katla and Rashad.

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MOTHER TONGUE

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NIHALI PHONEMES

Roger Williams Wescott

Although MT-II and MT-III contain a wealth of articles on the origin and affiliations of Nihali, the Nihali phonemic system remains implicit throughout those publications. This contribution constitutes an effort to make that system explicit. It is based primarily, but not exclusively, on Asha Mundlay's "Nihali Lexicon" (MT-II, pp.17-40).

A maximally compact roster of Nihali phonemes follows. (The diagram is schematically rather than phonetically, arranged.)

labial	apical	palatal	velar	dorsal
ph	th	ch	kh	
p	t	c	k	ʔ
b	d	j	g	
bh	dh	jh	gh	
	s			h
m	n			
	r	l		
w		y		
u	e	i	a	o

To these segmental phonemes, the three prosodic phonemes of vowel length, nuclear nasalization, and high tone should be added. This means that any Nihali vowel may be either short or long, oral or nasal, low pitched or high pitched.

The four digraphs ph, th, ch, kh presumably represent monophonemic aspirated stops rather than diphonemic clusters of stop plus fricative. And the four digraphs bh, dh, jh, gh are presumably voiced correspondents of ph, th, ch, and kh.

The two digraphs mh and nh may represent either aspirated nasals or word-onset clusters. (Context gives no clue. But typology makes the cluster interpretation more plausible as well as phonemically more parsimonious.)

The consonants ṭh, ṭ, ḍ, ṇ are presumably retroflexed apicals of the type traditionally termed cerebrals or cacuminals. To these, Sudhibhushan Bhattacharya adds the retroflexed rhotic ṛ. All of these retracted apicals are apparently either free variants or dialectal variants of common, or unretracted, apicals.

ć and č are apparently free variants of c, although it is unclear whether the variation here is phonetic or graphemic. (Oddly, j' and ǰ do not occur.) š is apparently a free variant of s.

The velar nasal η never occurs word-initially. The semi-vocalic glides y and w and the glottal stop ʔ never occur word-initially except as variants of the phonic zero preceding word-initial vowels. In this position, w has v as a variant.

The only word-initial clusters consist of consonants plus h or consonant plus y. The only word-final cluster is -yn.

Intervocalic biconsonantal clusters are common and include geminates. All intervocalic triconsonantal clusters end in y, except for the sequence ngr (which may be a misprint for ηgr).

Sten Konow includes a nasal written as ṅ (in peṅg, `head'). Here the digraph ng may be an alternative way of writing the nasal velar η.[1]

Much phonetic detail regarding the articulatory and acoustic realizations of the above phonemes and their graphic variants is needed. For example: are the apical consonants dentals, alveolars, or free variants of the two options? Are c and j palatal stops or prepalatal affricates? Where do ć and č stand in this range? Are bh, dh, jh and gh aspirated or murmured? Is š retroflexed or prepalatal? Is the rhotic r a tap, a trill, or an approximant? Is the lateral l velarized or unvelarized? Is the v variant of w a bilabial fricative or a labio-dental fricative? Only field-workers like Mundlay, Bhattacharya, and Konow are likely to be able to answer these questions

Putting aside allophonic variants for the moment, I arrive at a tentative inventory of 34 phonemes (31 segmental and 3 prosodic) for Nihali. But, if mh and nh turn out to be aspirated nasals, the inventory will increase to 36. Moreover, if our field-workers discover or decide that such presumptive allophones as the retroflex series or ć, č, š, v, and η should be granted phonemic status, the total phonemic inventory could reach 46. We look forward to their input.[2]

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[1] Editor's Note: It is so. The use of a dot over an [n] to represent the velar nasal[η]has been very common in European phonetics.

[2] Editor's Note: This may prove taxing to Sten Konow who has had 90 years to improve his phonetics (cf. Konow, 1908). One suspects that he has become hearing-impaired by now.

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Some Comments on Ilia Peiros' "Nihali and Austroasiatic".

John D. Bengtson

We are fortunate to have had the thoughtful comments of Ilia Peiros on the subject of Nihali. However, after careful consideration, I found that this article (Peiros 1996: henceforth N&A) did not (and, due to the author's constraints, could not) tell the whole story of the proposed relationship between Nihali and Austric (Bengtson 1996, Blazhek 1996), for the following reasons: 1) Some known Nihali words are not cited by Peiros; 2) Loanwords are cited rather than native words; 3) Plausible comparisons are overlooked; 4) The selection of Austroasiatic languages is very narrow; 5) Evidence from other Austric languages is not considered. Each reason will be discussed in more detail below.

1. Some known Nihali words are not cited. These words include some that are found only in the earlier glossaries by Konow and Bhattachar-ya, but also some that are found in Asha Mundlay's lexicon, the material of the N&A. For example, \`belly' is left blank, but we have Mundlay's entry 1342, popo \`belly', which is comparable with the Sora, Mon, and Khmer words cited in N&A; entry 479, ḍelen \`to drink' (not similar to any of N&A's lexicon); entry 1581, ṭé \`to eat' (not similar); entry 205, béʔ , béthe, etc. negative morpheme, which is comparable with some Munda languages not cited in N&A, viz. Santali-Karmali ba \`not', baŋ \`no', Korwa bai \`no'; more remotely, Khmer bŭ̃(pŭ̃m) \`ne pas, ne, non'; entry 709, hoʔ negative morpheme (and Bhattacharya's hoṭ with sandhi forms hoc, hop, hok), comparable with Mon ha, hwa and Khmer ʔot of N&A; cf. also Nicobarese hət\`not'. In some cases the lack of known Nihali words makes no difference, but in the case of the negative morphemes, the omission is serious.[3]

Of words available mainly from the glossaries by Konow and Bhattacharya, we have for example Nihali parka \`all', comparable with Korku par (loanword in Nihali, or in both languages?); Nihali pakoṭo \`bone', comparable only with Mon cut, but considered of Dravidian origin by Kuiper (1966); oṭṭi- \`to burn', not comparable with N&A, considered native by Kuiper; pi- (pa-) \`to come', not comparable with N&A, but cf Gutob pe, pi(ŋ) \`come', Semang Ijok peh,[4] etc.; āpo \`fire' (cf. 65-66 of Mundlay), not comparable with N&A, but surely with Austronesian *hapuy \`fire', Ainu *apOy, etc.; khuri \`foot', not comparable, and according to Kuiper a loanword from Kurku; boko, bokko \`hand', not comparable with N&A but part of an extensive global etymology; mancho \`man' (Indo-Aryan or Dravidian?); kharuka \`many' (Korku); ghane \`many' (Korku ghonej); iphil \`star' (similar to Mundari ipil); hi, i \`this': cf. Kharia hin, Kaseng hɛ, Halang hi \`this', etc. Of these the most serious omissions (for wider connections) are \`come' and \`fire'.

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[3] Editor's Note: Just to clarify. Negative morpheme here = a morpheme which represents negation in a particular language.

[4] Or even *pay-i \`to go', in Ainu , a language possibly related to Austric.

2. Loanwords are cited rather than native words. There is one questionable case, and another more serious. Nihali baṛa `big' (Indo-Aryan loanword) is cited rather than bhāgā `big, much', of uncertain origin. The more serious case is Nihali bai `woman', cited rather than the native kol `woman', comparable with Mundari kuri (opposed to kora `man'), and other Austroasiatic and Austric words such as Mon krŭə̣ `male', Austronesian *hulang `man (human being)',[5]. Cf. also Nihali kol-ṭa / kal-ṭo / kal-ṭu-manḍi `Nihals / Nihali person / Nihali language'.

3. Plausible comparisons are overlooked. Of course, as long as there are no rigid rules, the judgment of which words are similar to which others is to some degree subjective. In some cases, N&A rejected comparisons that seem more or less plausible to me. For example, Nihali poy `bird' and Bonda piriʔ; Nihali pater `dry' and Mon parom; Nihali poye `feather' and Bonda bileʔ; Nihali cokob `leaf' and Mundari sakam; Nihali cilar- `louse' and Mon cay; Nihali biḍ, etc.`one' and Mundari mid, etc.; Nihali tól `skin' and Sora usá:l; Nihali enga `that' and Mon ʔigah; Nihali menge `tooth' and Mon ŋek; Nihali aḍḍo (~ arḍu) `tree' and Sora ará:; Nihali nāni `who' and Khmer ʔanakna. Perhaps Peiros' deeper knowledge of the languages concerned allowed him to reject some of these comparisons.

4. The selection of Austroasiatic languages is very narrow. N&A uses two North Munda languages (Mundari and Korku), two South Munda languages (Sora and Bonda or Remo), one South Mon-Khmer language (Mon), and one East Mon-Khmer language (Khmer). Of the sixteen genetic nodes found in Peiros' Austroasiatic classification, only three are represented in N&A: Mon, Khmer, and Munda. If we follow the chart given by Ruhlen (1987: 334-337), which divides Mon-Khmer into three major divisions, East MK is represented by one language out of 72 (Khmer), South MK by one of 23 (Mon), and North MK (43 languages) by none. Perhaps adding a few more languages (e.g., Khasi, Vietnamese, Nicobarese) would improve the balance.

5. Evidence from other Austric languages is not considered. Since Ilia Peiros (1992) is on record as a supporter of the Austric hypothe-sis (Austroasiatic + Miao-Yao + Kadai + Austro-nesian), I expected there might be some mention of the possibility of Nihali lexical parallels with the other three components of Austric. However, there was none. From my own rough notes, I suggest the following possible comparisons (also including Ainu, which I tentatively classify as Austric):

a) Nihali aḍḍo ~ arḍu `tree, wood'; Munda: Sora ará: `tree'; MK: Bru aru'ih `woods'.
b) Nihali bokki `to tie, bind'; AN *bekes, *buŋkus `bundle'; Formosan: Siraya vugot `to bind'; Tagalog bigkis `bundle, waistband, bound, to tie, etc.
c) Nihali bologo `bear' (Ursus); AN *ba[l̥]uva¤ = *ba/r/uwaN `bear'.(Sanskrit bhallūka- from Old Nihali?)
d) Nihali bóy `grass, reed, weed,' etc.; MK: Katu bo'i `grass'; AN *babaw `weed'.
e) Nihali cākini (LSI cikn-) `to hear', cigam `ear'; Munda: Kurku cina`to recognize, know'; MK: Katuic *saŋ `to hear'; Nanhang chong `to listen'; Bahnaric kâtin `to hear'; Tembi, Serau, Darat entak^{n} etc.

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[5] And even Ainu *kur `person, man'.

f) Nihali cán `fish'; AN *hikan `fish'.
g) Nihali cáto `hunger, be hungry'; AN: Oceanic sa(dr)a `hungry'.
h) Nihali ceki- `to catch hold of, to hold, arrest, catch'; AN: Polynesian *siko `to take hold of'.
i) Nihali cokob `leaf of a tree'; Munda: Kurku, Santali, Mundari sakom `leaf'; MK: Katu aso'q `leaf'.
j) Nihali con, cojona `nose' (dial. `mouth'); AN *hijuŋ`nose'; Oceanic;*isu(ŋ) `nose'; Ainu etu `nose'; Ong Be zoŋ`nose'; Tai ʔdaŋ
k) Nihali édugo `(house) fly'; Munda: Kurku ruku, etc.; MK: Bahnaric *ruɔy `fly'; Kadai: Kam-Sui *ʔdlu `bee, wasp'.
l) Nihali joppo, jappo `water'; MK: Katuic ʔjip `wet'; AN: Philippine *sapa[ ] [sic] `river'.
m) Nihali kalen `egg'; AN: Polynesian *kala `testicles'; Kadai: Kam-Sui *krai `egg'; Miao-Yao *qyaw = *ql̥aw `egg'.
n) Nihali kurup `stone'; MK Sakai of Kerbu *gerbó: `hill, mountain', Tembi ge:rbu `hill, mountain'; AN: Oceanic *kor(a,o) = *gor(a,o) `mountain'; Ainu kuru `hill, mountain'.
o) Nihali lá `you (pl.)'; Kadai: Lakkia liu `you (pl.)'; Lao lau `thou'.
p) Nihali lege `up'; Munda: Kurku le:n `above, upon', Sora laŋka:-n `above'; MK: Khmer lîŋ `monter, s'élever, gravir', etc.; AN *laŋit `sky'; East Oceanic *laŋi `up'.
q) Nihali pada- `to kill'; AN *patay `to die'; East Formosan *paṭay `to kill, die'.
r) Nihali págo `tail'; AN *puŋku[l̥] `behind'; Fijian mbuku `pointed hind-end, tail'.
s) Nihali sika `pubic hair'; Munda: Kharia sɔʔ-lui `hair'; MK: Mon, Khmer sok `hair (of head)', etc.

Twenty-six other Nihali-Austric parallels are listed in my article "Nihali and Ainu", in Mother Tongue II; see also Václav Blazhek's "Seeking the Relatives of Nihali", in the same volume.

Since N&A was restricted by the author to (a small number of) Austroasiatic languages, the cards were stacked against detecting any Austric affinity of Nihali. Blazhek and I agree that Nihali is not sensu strictu Austroasiatic, but we also find that linguistic evidence points strongly in the direction of an Austric macro-family that includes Nihali and Ainu, as well as the four families (Austroasiatic, Miao-Yao, Kadai, Austronesian) already posited by Peiros.

ABBREVIATIONS: AN = Austronesian MK = Mon-Khmer

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CORRECTIONS: Following are some corrections of Ilia Peiros' article "Nihali and Austroasiatic" (MT II, pp.75-90):

p. 78, no.4 \`belly': "Mon 4, Khmer 5" should be "Mon 2, Khmer 4"
p. 84, no.56: \`month' should be \`mouth'
p. 88, no.89 \`tooth': "Khmer 5" should be "Khmer 6"
p. 90, no.100 \`yellow': "Mundari -, Korku -, Mon 2, Khmer 3" should be "Mundari 1, Korku 2, Mon 3, Khmer 4"

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"Call it Methodology!" Comments on the Strategy of Peiros' Paper.

Harold C. Fleming (Gloucester, Mass.)

In examining the relationship between Nihali and the Austric super-phylum of languages, Ilia Peiros evidently thought to focus the research on those lexical items most likely to produce results -- the basic vocabulary represented on the Swadesh list. As everyone knows, or is supposed to know, the most conservative lexical items are most resistant to borrowing, most apt to show basic sound correspondences, and most often present in dictionaries, glossaries or word lists. Therefore, if one wishes to find cognates, one must compare a few lists of basic words as found on the standard Swadesh 100-list.

Dr. Peiros did all that and found, perhaps to his surprise, that cognates were scarcer than hens' teeth. Therefore he concluded that Nihali was not related to the Austric super-phylum. It is very very interesting that Paul Benedict used precisely the same argument to deny the Austric super-phylum which Peiros believes in. Scarcer than hens' teeth are the cognates, said Paul Benedict, among Kadai, Austronesian, Austroasiatic and Miao-Yao languages. So Peiros must have found cognates which the massive erudition of Paul Benedict had missed? Not quite, because Benedict found **look-alikes** which he denied cognate status to on the grounds that they were **borrowings!**

Yet the fact that Benedict was able to find look-alikes was due not only to the greater phylogenetic proximity of languages within Austric but also because he did not limit his research to Swadesh 100-word lists. No doubt it is useful in mass comparisons to line up data by Swadesh meanings but it is a serious mistake to apply **glottochronological reasoning** to the comparisons. Such reasoning grossly restricts the search for cognates. Among other things it is unable to transcend the requirement of exact same or same meaning. One cannot substitute \`smell' for \`nose', or \`ear' for \`hear' if one proceeds Swadeshly. A certain percentage of conservative items have been replaced, or more likely displaced, in any language. The familiar \`hound' for \`dog' in English is an example. Amharic keeps the old Semitic word for \`moon' as /wâr/, meaning only \`month', while another old one survives only in \`kneel' /tân-bârâkkâkâ/ and \`wobbly (knees)' /tâ-brâkârṛâkâ/ from old Semitic /*b-r-k/ \`knee'. There are many of these \`lost' conservatives. One of the cutest is Ongota's [č›a^{c}a] \`stone' which is cognate -- by sound correspondences -- with Somali [d›a$^{c}$a] \`sound made by a falling stone', as opposed to regular /d›agaH/ \`stone'. Neat, what?

A key consideration is this: a cognate is a morpheme linked to another by descent from a common ancestral morpheme. It is not defined as a word having the same meaning and same form as another. But on the Swadesh list items are considered cognate if they have the same meaning and similar (hopefully cognate) forms. Thus the famous Armenian case of /yergu/ `two' which is cognate with, but has a dissimilar form from Urdu /do/. But, lest we lapse into Indo-Baloney, it is refreshing to remember that most cognates look a lot like their mates in other languages. At least it seems that way to me, after many years in this business. Or a high percentage of look-alikes are also real cognates. What is a similarity depends on the scholar. `Similarity is in the eye of the beholder.' Peace to all methodology freaks!

To quote Isidor Dyen: "I'll search everywhere to find cognates."

Dr. Peiros has confused the **cognate etymology hunt** with our old pal Morris Swadesh's glottochronology and its brother, lexicostatistics.

SPECIAL TOPIC II: SUMERIAN

This section was initiated by the submission to Mother Tongue of an article, "External Connections of the Sumerian Language," by Igor M. Diakonoff (St. Petersburg, Russia). Diakonoff, an ASLIP Council Fellow, is recognized around the world as a leading authority on the extinct languages of the Middle East, as well as on the Afro-Asiatic or Afrasian language family (e.g., Afrasian Languages, Moscow, 1988). Although his comparison of Sumerian with Munda has taken many long-rangers by surprise, his thoughts deserve careful consideration.

The other two articles in the Sumerian section reiterate ideas already known to many readers. Allan Bomhard compares Sumerian with Nostratic, stopping short of claiming that Sumerian is a Nostratic language. John Bengtson re-evaluates his earlier judgment

Gonzalo Rubio, who teaches Sumerian at Johns Hopkins University, has agreed to act as a discussant of the three articles on Sumerian. His comments, as well as responses by the three authors, will be featured in *Mother Tongue* IV..

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External Connections of the Sumerian Language

Igor M. Diakonoff

The idea of the unity of the Austroasiatic language family -- or, rather macro-family, including the separate(but related) families Mon-Khmer, Munda[61], Sumerian, and perhaps even Australian -- was launched as early as the beginning of this century by Wilhelm Schmidt, but later, in the middle of the century, was rejected, at least as regards Sumerian, by authoritative linguists. However, it seems to me that the question deserves closer study. I have tried to re-examine at least the possible relation between Sumerian and Munda, especially Kherwari, mainly Santali and Mundari.

The possible relations of Sumerian with other languages is one of the unsolved riddles in linguistic scholarship. Being (along with Old Egyptian) one of the world's first languages which has ever been fixed in writing, Sumerian stands alone, and no languages related to it have as yet been identified. This is a rather puzzling situation, actually a riddle, which I will attempt to solve, at least in part.

Sumerian myths seem to point to the island of Dilmun in the Persian Gulf as the homeland of the Sumerians. This is not very probable, because Dilmun (modern Bahrein) is nothing but a big, partly bare, rock, and seems not at all to be suited for the role of the homeland of an important civilization. The Dilmun myth should probably be treated as a remembrance of the arrival of the Sumerians from the East along the Persian Gulf; hence, we may be allowed to make at least an attempt to find some linguistic contacts with some peoples to the east of the Dilmun rock. This means Southern India, now partly inhabited by the comparatively backward tribes of the Munda linguistic family, which formerly might quite possibly have inhabited the whole region of the Indian subcontinent, from which they were driven by the Dravidians, now (to my mind, and in spite of much criticism from traditionalist Dravidologists) securely enough proven to be somewhat distantly related to the Elamites. (The Brahui language in Baluchistan seems to be a link between Dravidian and Elamite). It was, possibly, the Dravidians who created the Harappa culture in Nortwestern India during the IIIrd millennium B.C.E. (The Harappan script is still undeciphered.) The Harappans, in their turn, were followed onto the subcontinent by the Indo-Aryans.

Trying to look for possible Sumerian-Minda linguistic relations, we must keep in mind that the earliest Sumerian texts have come down to us from ca.3000 B.C.E., and a pre-literate period during an earlier sojourn of the Sumerians in Southern Iraq is very probable. However, in the Xth-VIIth millennia B.C.E. the plain of southern Iraq, like the territory of the Delta of the Nile,[7] was covered with water, and later with swamps, before

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[66] Editor's Note: We have spelled Munda in its ordinary English manner. In the author's manuscript it shows retroflex dots under the [n] and [d]. The same remarks apply to Mundari.

[7] See I.M.Diakonoff, 1996. "Some Reflections on the Afrasian Linguistic Macrofamily", JNES, 55, No.4, p.294, n.3.

finally the plain became a fertile lowland watered by the Tigris and the Euphrates. The date of the colonization of Lower Iraq by the Sumerians should hence be not earlier than the VIth millennium B.C.E. The date of the possible sojourn of Sumerians in Southern India might have been somewhat earlier. This means that the time distance between modern Munda and the loss of Sumerian-Munda contacts would amount to no less than eight millennia.

A drastic lowering of the ocean level at about the middle of the VIth millennium B.C.E. may also have been the reason for the disappearance of the highly developed Çatal Hüyük culture in Asia Minor (and for the disappearance of the lakes and swamps, occupying until then most of the Nile Valley below Khartoum). Because of the increasing drought, the population wandered probably from Asia Minor over to the Balkans (where Terteria hieroglyphs were invented). Here in the fertile forests of the Balkan peninsula goats and cattle were domesticated, and the children were reared on a vegetable, milk and meat diet. This led to a vigorous dispersion of the tribes in question (probably the Proto-Indo-Europeans); the new tribes ousted the backward and scanty Megalithic tribes in Western Europe, and also started a Völkerwanderung towards Asia.

As to their physical anthropological type, pure Sumerians appear not to have been really so white-skinned as they look on some sculptures; on some of them they are sallow, but on wall-paintings they have red-brown skin; the hair is wavy. The present-day Munda are proto-Australoid, brown-skinned, and with wavy or curly hair.

This, however, need not trouble us too much, because language areas commonly do not coincide with racial areas. After all, dark-skinned Americans also speak Indo-European, and the immigrating Sumerians would as a matter of course mix with an alien substratum.

The (possibly) Munda-speaking population of the pre-Harappan Indian territory would probably also differ from the Munda of today's India, now actually surviving in minor enclaves.

The Munda linguistic family is divided into several branches: Northern Munda, including Korku and Kherwari (a group of dialects, of which the most important are Santali and Mundari; other dialects assigned to the Kherwari group are the Ho, Bhumij, Birhar, Turi, Asuri and Korwa). Kharia belongs to a Southern group. The languages Juang, Savara (Sora), Gorum, Gutob, Remo, Gta belong to the Southern group (or the Southeastern groups). These dialects have not so well preserved the crucial features of the Munda linguistic family type; they are more strongly influenced by outside languages. In the following, we shall base our reasoning chiefly on Kherwari (actually on Santali and Mundari), which seem to preserve the typical Munda features best of all.

The Sumerian verbal system is extremely complicated (see below), and differs strongly from the modern Munda verb. This is not crucial for the appreciation of a possible relationship between the two languages; thus, the English verbal system differs radically from the Russian one, in spite of the fact that both languages belong to the same Indo-European linguistic family.

The Sumerian language is ergative, with a special ergative case for the subject of a verb of action. The Munda verb distinguishes verbs of action and verbs of state, but no special ergative case. The Munda verb is actually a combination of nominal, pronominal and adverbial elements; what makes the whole combination a verb is the addition of the "categorical -a".

This curious grammatical phenomenon has, as far as I know, no analogy in other Austroasiatic languages (grammatically Munda has, on the whole, a different typology as compared to Mon-Khmer and other Austroasiatic languages), but it does have a close analogy in Sumerian, where -a is a relative element marking a noun as denoting a state (also a state resulting from an action), and a verb as predicate of a subordinate clause. Neither Sumerian nor Munda has gender.[8]

Munda has no x; Sumerian has no h.[9] Supposedly, for Munda h, Sumerian has zero. Munda has a phoneme which is spelled either as k or h in the different dialects; this may in some cases correspond to the Sumerian x; cf Santali ka-ku `fish' (-ku being a plural marker), Mundari ha-i `fish', Sumerian xa `fish' etc.

Sumerian has no less than six types of plural:
1) group plural (formally = singular);
2) definite plural (stem + plural marker -ene);
3) inclusive plural (implying all objects of that particular category by reduplication of the stem);
4) inclusive-definite plural (implying all definite objects of that particular category by reduplication of the stem + plural marker -ene);
5) distributive plural (name of the object + `one-one');
6) sorting plural (pointing out that the multiplicity of objects mentioned are divided into several distinctive sorts or types; expressed by adding the noun (or adjective) xá `sort, type' to the stem of the noun in the singular. It is possible that the typical Munda plural indicator, -ko or -ku, is cognate with the Sumerian xá.

The phonetic structure of a Munda word is not unlike that of Sumerian. There are no consonant clusters either at the beginning of a word-stem, or at the end; bisyllabic word stems usually are of a $V_1C_1V_1C_2$ or $C_1V_1C_2V_1C_3$ type; a V_2 in the second syllable is rather rare. The Sumerian lacks the series of retroflex consonants typical of Munda; apparently, these have coincided with the simple voiced or unvoiced consonants.[10] Munda has palatalized consonants č and ǰ which are lacking in Sumerian, but correspond to Sumerian **z**, whatever the actual pronunciation of the latter was.

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[8] See Bhattacharya (1976), pp.189-212, who points out expressions like `mother tiger' for `tigress', or `father goat' for `billy goat', and terms this phenomenon `grammatical gender'. A somewhat similar phenomenon can be noticed in Sumerian, where the noun igi `eye' is used as a preposition `before'.

[9] Editor's Note: We use currently preferred [x] for the velar fricative, instead of the author's [ḫ].

[10] Unless the retroflex consonants reflect the proximity of a

On the other hand, Sumerian has a series of sibilants, conventionally transcribed by the assyriologist as **z**, **s** and **š**. This transcription is almost certainly wrong: I had suggested that they should be read **s**, **ś**,**ŝ**, but my arguments were apparently not convincing. Now it appears that the supposed Sumerian **z** corresponds to Santali **ǰ** and **č**. Kherwari has only one simple sibilant, **s**, corresponding to the Sumerian conventional **s**.

Kherwari has several nasals: **m**, **n**, **ṇ** (retroflex), **ñ** (palatal), **n'** (glottalized)[11]. However, **ṇ** and **n'** appear (at least in Mundari) only in non-initial position, so that **ñ** in initial position stands also for **ṇ** and **n'**. Sumerian has three nasals: **m**, **n** and **g̃** **(or ŋ)**, but the last phoneme was tabooed for women, and does not appear in the "women's language" Eme-sal (ES), where the m was pronounced instead. This "women's " pronunciation had in a few cases infiltrated also in he Sumerian men's language Eme-gir.

Sumerian was certainly a tonal language, which is proved by the very numerous homonyms. Thus, there are more than forty words read as bar; in rather numerous cases the semantics of these syllables are quite different.[12]

The Munda languages do not distinguish (or have not retained) tones.

Only the following XXth century publications on the Munda languages were available to me:

1. P.O.Bodding,1929-. A Santal Dictionary vol.1, Oslo; vol.IV, Oslo, 1935-
2. Hans-Jürgen Pinnow, 1963. "The Position of the Munda Languages within the Austroasiatic Language Family", Linguistic Comparison In South East Asia and the Pacific, School of Oriental and African Studies, London.
3. Hans-Jürgen Pinnow, 1965. "Personal Pronouns in Austroasiatic": Indo-Pacific Linguistic Studies. Part I. Lingua, vol. 14, Amsterdam.
4. Arlene R.K.Zide and Norman H. Zide. 1976. Austroasiatic Studies, Part II, pp. 295-334. Honolulu.
5. Sudhidbhushan Bhattacharya, 1976. "Gender in the Munda Languages", Austroasiatic Studies,Part I, pp.189-212, Honolulu.

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former specific tone.

[11] Editor's Note: We have altered two of these symbols because they create confusion vis-a-vis current usage. For the retroflex he had an [n] with a dot over it. For the glottalized he had [ŋ] the usual symbol for a velar nasal.

[12] Editor's Note: It should be noted, however, that no language of the modern world has more than 10 tonemes. May we suggest that more than tone is involved in the huge number of homonyms?

6. Toshiki Osada, 1992. A Reference Grammar of Mundari. Tokyo.

In order to check the possible genetic connection of Sumerian and the Munda languages, we ought probably to use the Swadesh hundred-word list, which supposedly retains the desig-nations of objects and actions typically present in any civil-ization; according to Swadesh, 20% of the most usual vocables are lost with the passage of every one thousand years, and in each next millennium 20% of the preserved vocabulary is again lost. Since the distance in time between the Munda of the XX^th^ century A.D. and the arrival of the Sumerians in Iraq about 6000-5000 B.C.E. amounts to 7-8 millennia, the number of lexemes common to Munda (actually Kherwari) and Sumerian could be expected to be about 20. It must be noted, however, that in real history the lexemes preserved may not actually be only the ones selected by Swadesh for his list;,the continuation (or otherwise) of the life of a lexeme would heavily depend on chance events in the history of local material and spiritual culture, on outside influences, etc. Anyway, the approximate number of vocables retained from the proto-language after a period of separate existence of the dialects of some 8000 years, would be about 20, although such a great expert as S.A.Starostin is of the opinion that the retention of only 10% of the Swadesh list is sufficient to witness for the genetic connection of the languages in question.[13]

No amazing similarities are to be expected between Sumerian and the Munda languages, but some suggestive material may perhaps emerge. For Munda, we have used mostly Santali (and partly Mundari) material, as being seemingly the best preserved. Munda, like Sumerian, is a language which formally distinguishes verbs with an object from verbs which cannot have an object. However, the verbal structure is somewhat different from the ergative, so well known from many ancient Oriental languages, beginning with Sumerian.

A serious difference between Sumerian and Munda is that Munda lacks the typical Sumerian space-direction markers at the beginning of the verbal form, including the pronominal markers. But this phenomenon is not found in any known language except Sumerian, and is apt to be a Sumerian innovation.

Now let us try to show what really serious reasons for deliberation on Sumerian-Munda linguistic connections we have (if any such reasons can be brought forward at this preliminary stage):

Personal pronouns, 1st p. sg: Munda: **ŋ**, **Iŋ** , **ŋga**, etc.; Sumerian **aŋ(e)**, ES **ma(e)** (Note: **-e** is a relic of the ergative marker). -- 2nd p. sg., Munda **am (m-, me-)**; Sumerian **za(e)**. Cf Bolowen (Mon-Khmer) **sôu**, Niaham (Mon-Khmer) **ša**. --3rd p. sg.

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[13] S.A.Starostin "Comments on the Basque-Dene-Caucasian Comparisons", Mother Tongue II, 1996, p.102 : "Statistical considerations tell us that to be considered related, languages must have no less than 10% of their most basic vocabulary in common (within Swadesh's 100-word list). If they have less, they may also be related (just separated too long ago), but a figure of 5% may also be due to a chance coincidence."

Munda is expressed by different demonstrative pronouns (e.g., Mundari **en-**); Sumerian **ane**, **ene**.

Possessive pronouns: 1st p. sg., Munda : **(i)ŋ** ; Sumerian -**ŋu**[14] **-mu**. 2nd p. sg. Munda: **ma**; Sumerian **-zu** (cf. the Bolowen and Niaham personal pronouns). 3rd p. sg. Munda **-a**; Sumerian -**ani** (animate), **-bi** (inanimate). Note: For 3rd p. dual and plural Munda can also use the dual mark **-kin**, and the plural mark **-ko**. Note: Sumerian has no dual, nor does Sumerian mark inclusive and exclusive plural, as does Munda.

Demonstrative pronouns: Munda: **ni**, **nia** and similar. Munda possesses also a series of demonstrative pronouns (sometimes used as 3rd person of the verb), of the type **hini**, **ini**, **inä**, reminding of Hurro-Urartian, but perhaps also connected with the Sumerian demonstrative pronoun **ene**. Note the 3rd p. personal pronoun **en-** in Mundari.

Any Munda noun can, in principle, also act as a verbal stem. A very important feature common to both languages is the verbal suffix **-ed** (Sumerian) or **-et** (Munda). In Sumerian this suffix signals that the verbal action in question is possible or imminent in the future; in Munda it marks the present tense.

In Munda, an **-a** marks the end of any verbal sentence ("categorical **-a**"); in Sumerian **-a** ends a subordinate clause. In both languages a very important role is played by the postpositions, corresponding to the European pronouns: Munda (we quote mainly from Santali and Munda-ri): **-ak'** **(-a')** genitive; adjective final mark: **reak'**, **rean**, **ren** do., for inanimate nouns. The use of the **-ak'**case with animate or with inanimate nouns seems to be not quite standardized through the dialects, although here I may be in error. Furthermore: Mundari **-re** \`in', **-te** \`to, by', **-ate**, **-ete** \`from, after', **-lo'** \`with, while, along', **-a'** \`possessive for animate nouns, **-sa'/k** \`from, on the side', **-ta'/k** \`near', **-ko** \`approximate'; Santali **-ak'** \`possessive', **-re** \`in', **-te** \`in, into, by means of', **-ate**, **-ete** \`from', **-lak'** \`with, together', etc.

Sumerian" **-a-** \`locative' (in), **-(e-)ra-**, **-(e-)re-**, **\*-n-ra- > -na-** \`dative' (to, for), **-da-**, etc. \`comitative' (with), **-ta-**, etc. \`ablative' (from), **-eše-**, **-ši-** \`allative' (to, towards).

Here follows a list of words common to Sumerian and Kherwari:

Comparative Lexical List

Sumerian	Kherwari
1. -a marker of dependent clause passive participle	1. -a marker of all c conjugated verbal forms
2. a, ad \`nearest kin' [Lallwort]	2. a, aba, ad grandfather, nearest kin'

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[14] [a], [e] in a syllable before [u], [i] is pronounced otherwise than [a] in other positions, but this has no etymological value.

3. aba, apa, apu, ad `father'	3. apa, ap/bu `father'
4. ad `dead person, forefather'	4. (vide #2)
5. ag `make, do'	5. agu- `bring'
6. -ak' `genitive marker'	6. -ak', -a `genitive marker (for animate or inanimate in different languages)
7. am- III (rare!) `gift, present'	7. Vm-/om-/am- etc `give'
8. ama `mother' (possibly originally an Eme-sal form <-- EK *V_a?)	8. (V)ηa (Sa.), ema (Mu.) `mother'
9. áš `wish, curse'	9. as (Sa.) `bring, fetch, get'
10. bar `one half'	10. bar (Sa.) `two, a pair'
11. baz `cripple, dwarf'	11. bač (Sa) `rejected, refuse'
12. bùluη V `malt'	12. buluη (Sa) `slime, sweat, etc.'
13. bùru `vault, dome, silo'	13. buru `mountain, hill'
14. -da `with'	14. do (Mu.) topic marker
15. dag / dadag `clean, washed '	15. dak (Sa.) da (Mu.) `water, rain'
16. ene, ane `3rd p. pronoun'	16. en- (Mu.) `3rd p. pronoun'
17. gal `big, great, manyfold'	17. gal (Sa.) `ten, decade'
18. gaz `to kill'	18. gač, gaj (Sa.) `to kill, to be killed, to die'
19. ηa(e) `I'[15]	19. (a)iη, Vη, etc. `I'
20. ηu$_{10}$, mu `mine, my'	20. -η `my'

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[15] **-e** is a fossilized form of the ergative case.

21. xa `fish'	21. ka-ku (Sa. -ku pl. marker), ha-i (Mu.) `fish'
22. lag `chip, piece, clump'	22. lag/k `(Sa.) `piece, clump'
23. líl, lil `obsessed'	23. lelha (Sa.) `idiot'
24. lum `fertile'	24. lum (Mu.) `wet'
25. -zu `thy'	25. -m `thy' (but cf. **sô** Bolowen in Mon-Khmer)
26. me `to be'	26. mena(') (Sa.) `to exist' [<Indo-Aryan?]
27. men `to agree, allow'	27. men (Sa.) `to mind, warn'
28. ηi(g) `black, night'	28. ηinda (Sa.) ñida (Mu.) `night' possibly < Su. ηI(g)-ná(d) `dark+sleep'
29. ηir `foot, leg'	29. ñir (Mu.) `to run'
30. ηu$_{10}$ (ES), mu (EK) `name'	30. num (Sa.) `to name', nu-tu-m `name'
31. ra `to' [16] `dative'	31. ra (Sa.) `in, within', ra < ra+an, re-ak' `genitive inanimate'; re (Mu.) `in, into'
32. sim `swallow'	32. sim (diff. dialects) `small bird, chicken, cock'
33. sur$_x$ `dog; warrior', etc. [17]	33. sor `dog' (thus in several Munda languages, not in Santali and Mundari)
34. -ta `from', `ablative'	34. ta', -ta `into, to, by means of' (Sa., Mu.) -ate, -ete `from' (Mu.).

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[16] Note that all postpositions in Munda have a broader range of meanings than their analogues in Sumerian.

[17] Traditionally read **ur**: **Ur-^{d}Nammu**, **Ur-^{d}Ba-ú**, etc. Lately proved by Sollberger to have been read **sur$_x$**.

I realize that I may not have proved my theses to every-body's satisfaction, but perhaps I have given my colleagues matter to think about. Surely, after all, every man and woman come from the same stock: the question is only the distance in the relationship. For more than a hundred years we have had no answer to the question: Where did the Sumerians come from? I do not presume to have found the final solution, but at least we have made a start looking for the needed answer.

St. Petersburg, Russia, July 8, 1997

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MOTHER TONGUE
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The Riddle of SUMERIAN: A Dene-Caucasic Language?

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The problem of the genetic affinity of the Sumerian language (along with all other supposed \`isolates') has deeply interested me for many years. By about 1989, I had come to the preliminary hypothesis, based on multilateral comparisons, that Sumerian was probably most closely related to the Dene-Caucasic languages (Basque + (North) Caucasic + Burushic + Sino-Tibetan + Yeniseian + Na-Dene; see, e.g., Bengtson 1991a, 1991b, 1992; Blazhek & Bengtson 1995).

I had not worked intensively on this problem during the past several years, but recently Diakonoff's paper (in this volume) impelled me to re-examine the riddle of genetic connections (if any) of our oldest written \`isolate'.

LEXICAL EVIDENCE: As part of the re-examination, I went through all the Sumerian-Dene-Caucasic etymologies in the articles cited above, in my unpublished notes, and some other materials (e.g., Ball 1918; Boisson 1990), rejecting some, modifying others, and adding some "new" comparisons. (One of the latter was actually proposed as long ago as 1918 (Ball, 1918), i.e., §6. DAY, with Sumerian ud and Tibetan od; another comparison cited by Ball: §4. BURN, Sumerian tab and Tibetan thab, was independently discovered by me (Bengtson 1991a, p.97, no.63) before I knew Ball's article) At the end of this process, there was a list of 41 etymologies I considered the strongest or most promising:

1. BLOOD Sumerian gur, gurun, kurun, kurum
    Basque gorri \`red', gorringo \`egg yolk'
    Na-Dene: PND *Gay \`blood': Haida Gáy ~ $^{c}$áy \`blood'; Chipewyan -gài \`white', -gày-í \`reddish' (Na-Dene *-y- from *-r-)
2. BREAK Sumerian pad.r \`to break' (phonetic [phatl], per Boisson 1989a) [18]
    Burushic -phalt- \`to break, burst' (from *patl- or *phatl-)
3. BREAST Sumerian agan \`breast (♀)'
    Yeniseian: Kott xanti \`breast, chest', ?proto-Yen.*qan-
    Na-Dene: Haida qán \`(human) chest'
4. BURN Sumerian tab
    Sino-Tibetan: P-ST *tǎp \`fireplace'; Tibetan thab, etc.
    Na-Dene: ? Tlingit t'ày \`heat, hot springs', -t'á \`to be hot,ripe', Chipewyan -t'ɛ̀ \`it is hot, to be

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[18] Editor's Note: Original was written [p^{h}at^{l}], an aspirated [p] and a lateral affricate. We write both simply as [ph] and [tl].

roasted' (few labials in Na-Dene)
5. COLD Sumerian te, ten `(be) cold', en-tena `cold, chill, frost'.
Na-Dene: P-ND *tin `be cold, freeze'; Chipewyan -tə̀n `to freeze; ice'; Beaver es-təne `ice', etc.
6. DAY Sumerian ud, u$_4$ `day', ud, utu `sun'.
Basque uda `summer'
Sino-Tibetan: P-ST ʔwa(t) `light, shine'; Tibetan od `light,shine, brightness'.
7. DIE Sumerian gam
Sino-Tibetan: P-ST *ghuam `to die, pine away'; Tibetan ãgum `to die (elegant)'.
8. DRINK Sumerian nag̃ , na$_8$ (phonetic [naŋ], [nã] ?)
Na-Dene: P-ND *na ; Tlingit d-ná; Mattole -ṇaah, -naan, -naaʔn̄, -naal, -naŋʔ 7; Haida niiɬ
9. EARTH Sumerian ki, gi, gú
Na-Dene: Haida q'wíí `soil'
10.FIRE Sumerian izi // še `fire', i-izi `smoke'
Basque su
Caucasic:[19] P-NC *c'ăjĦ̃; Chechen c'e, Lak c'u, Abkhaz á-m-ca
Burushic šu- (in šu-tum `hearth, fireplace')
Sino-Tibetan: P-ST *cu `to burn, bake'; Burmese čhu `to boil (intr.), to bubble'
Na-Dene: (*c'e-); Haida c'aaʔano ~ c'áánuu `fire' compound?) Tlingit s'eq, s'iq `smoke'; Chipewyan -tθ'àγ `there is smoke, to be smoky', etc.
11.FLY,TO Sumerian dal, dal-dal `to fly'
Burushic dal `above, up (wards), dal `to raise, take up'.
Na-Dene: Sarsi -dàl, -dáál, -dìɬ, -dǐɬč `to fly', Chipewyan -dɛ́ɬ , -dɛ̀l, -diɬ (several objects) fly'.
12.FULL Sumerian ús, uš$_4$
Basque oso `whole, complete, healthy'.
Caucasic: P-NC *=ɦŏc'V `full, fill'; Chechen =uz-na `full'; Khinalug c'i `full, fill'
Yeniseian: P-YEN *7ute `full'
Na-Dene: ? Sarsi -c'ìst `to be full, to fill'
13.GIVE Sumerian mu
Basque eman ~ emon (e-ma/o-n)
14.HAND Sumerian tibir

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[19] Editor's Note: The reader is reminded that throughout this paper the symbol [j] in Caucasic and Sino-Tibetan represents [y] as written by other conventions.

Caucasic: P-EC *t'wɨ̆bi `finger'; Dargwa t'up'
Yeniseian: P-YEN *təp- `finger' (in *təp-alči `thimble')

15. I Sumerian g̃á-, g̃in = [ŋa], [ŋin] // me
Basque ni
Caucasic: P-EC *nɨ̆:, Lak na; Dargwa nu
Sino-Tibetan: P-ST *ŋā- `I, we'; Tibetan ŋa

16.KNEE Sumerian dùg, du_8, du_{10} // zeb
Burushic: (H) -dúmus, (Y) -núŋus `knee'.
Sino-Tibetan: P-ST *t(r)ŭH ~ *d(r)ŭH `knee, elbow'; Burmese duh `knee', etc. [20]
Na-Dene: ? Sarsi -duu `to be bent, convex-like'

17.LIE Sumerian lu `to lie down, sleep, stand' (!)
Basque lo `sleep' (noun), lo egin `to sleep'
Caucasic: P-NC *=HVwt̲l̲ʼān; Akhwakh ƚ̄lʼuni- `to sleep'

18.LIFE Sumerian dal `Atem, Leben, Seele = breath, life, soul'
Basque odol (o-dol) `blood'.
Sino-Tibetan: P-ST *t(h)əlH `meat, flesh'; Lushai tāl `flesh, muscle'.
Na-Dene: Eyak deh̲l̲ `blood'; Navaho diɬ ; Galice dal, etc. `blood'

19.LIP Sumerian sù
Sino-Tibetan: P-ST *ć̆ŏj `beak'; Tibetan m-čhu `lip, beak'

20.LIVE Sumerian tin, tìl, ti, ti-la `to live'
Sino-Tibetan: P-ST *ti `reverence, honor'; Tibetan s-ti `to honor; rest, repose'
Na-Dene: Haida -tay, -tii `to lie' [sic]; Tlingit -tii `to be'; Eyak -tĩ `to live'; Sarsi -təh, -tí(n), -təɬ `to handle a living being'.

21.LIVER$_1$ Sumerian bà
Sino-Tibetan: P-ST *phia `spleen'; Thakur -pəy `liver'
Yeniseian: P-YEN *b[a]jbVl `kidney' (compound?)

22.LIVER$_2$ Sumerian kin
Basque kun- (in kuntzurrun `kidney' -- Bizkaian and Gipuzkoan variant)
Caucasic: P-EC *k̲unHV `kidney'; Chamalal k'ũ: `liver, etc.'
Burushic -ken `liver'
Sino-Tibetan: P-ST *kjVnH; Old Chinese *ginʔ `kidney'

23.MAKE Sumerian ag `to make, do'
Basque egin (e-gi-n) `to make, do'

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[20] Editor's Note: The uncertainty principle rules here. The initial segment of the P-ST form is either t, tr, d, or dr.

Caucasic: P-NC *=H͟q͟ `to do, make; be, become'; Agul aq'-`to do, make', etc.
Yeniseian: P-YEN *-aq- `to take, hold'; Kott ba-ʔək-ŋ `halten'

24.MEAT Sumerian su, uzu
Caucasic: P-NC *jəəmcoo `bull, ox'; Andi unso `ox', Ubykh cwə `ox, bull', etc.
Sino-Tibetan: P-ST *chu`cow, bull'; Tibetan m-dzo `yak-cow hybrid', etc.
Yeniseian: P-YEN *ʔise `meat'; Ket īś`meat, fish'

25.MOUTH Sumerian ka, kag / kak
Sino-Tibetan: P-ST *Qa: ; Tibetan kha, etc. `mouth'
Na-Dene: P-ND *ʔaʔ-qa `mouth'; Tlingit q'á ~ X'á; Chasta Costa ɣaa-, ɣa- `mouth' (in compounds)

26.NAME$_1$ Sumerian mu [phonetic [mũ]? cf. Boisson 1989a
Sino-Tibetan: P-ST *măIŋ ; Bodo muŋ , Dimasa mu

27.NAME$_2$ Sumerian sa$_4$ [denoting [sã]?) `to name, call by name'
Basque izen ~ uzen (i/u-zen) `name'
Burushic sén-, sí-, sén-as `to say, name / ditto / named'
Sino-Tibetan: Old Chinese *seŋ
Na-Dene: Tlingit sà `name, voice', -sàà, -sáá, -sén `to name'

28.NOT Sumerian la-, li- (negation used before verb prefixes /ba-/,/bi-/).
Na-Dene: P-ND *lV- (negative-prohibitive); Tlingit l `not'; Chasta Costa ɬa, ɬá `don't!', etc.

29.OLD Sumerian sumun, sun
Caucasic: P-NC *ś̡wäni `year'; Avar son, Lak šin, etc.
Sino-Tibetan: ? P-ST *s-niŋ `year'; Jingpo šâniŋ
Yeniseian: P-YEN *siṇ′ `old'; Ket ś̡In′, etc.
Na-Dene: Tlingit šàn `old person'; Navajo sán `old', etc.

30.RAIN Sumerian šèg̃ (phonetic [šeŋ]
Sino-Tibetan: P-ST *[ʒ́]āŋ `rain, shower'; Lushai č̡hēŋ `to rain continuously', etc.

31.SAY Sumerian di
Sino-Tibetan: P-ST *ti `to do, say, think'; Tibetan g-te `to say'
Na-Dene: Navajo -tiʔ `to talk'; Chipewyan -tə̀i `word, language, to speak'

32.SINEW Sumerian sa `sinew, rope'
Basque zain ~ zañ `vein, nerve, root'
Caucasic: P-EC *s̲ē̲ħ̲mV / *ħēms̲V `muscle, vein, intestines'; Akhwakh sse `sinew, muscle'
Sino-Tibetan: P-ST *(r-)sa `sinew, tendon, thread'; Tibetan r-ca `vein, root', etc.

33.SKIN Sumerian kuš
Caucasic: P-EC *q›weč›V `wineskin, leather sack';
Tsakhur q›oč›e `wineskin'
Na-Dene: Haida q'uc ~ q'oc `bark'

34.STAR Sumerian mul, múl, mul$_4$ `star', mul `to shine'
Caucasic: P-NC *mīɬwă `sun, day'; Chamalal miɬ

35.STONE Sumerian za (phonetic [tsa] = ca ?: Boisson 1989a
Na-Dene: Eyak caa, P-Athabascan *ce; Navajo cé

36.WASH Sumerian lux `to be clean, to cleanse, wash',
làx `to wash oneself'
Sino-Tibetan: P-ST *lɨ̆w(H) `to wash'; Burmese hiyaw
`wash (clothes or hair)', etc.
Na-Dene: Chipewyan lɛ́, -lɛ̀, làɬ `to dive, swim under
`water'; Hupa -luu, -le `id.'

37.WATER Sumerian ér
Basque (h)ur
Caucasic: P-EC *ħwirɨ `lake, pond'; Lezgi wir ~ ür ~
hür (in dialects), etc.
Burushic: hur `wooden water conduit'.
Yeniseian: P-YEN *xur `water', *xura `wet', *xur
`rain'.

38.WHO?$_1$ Sumerian a-ba
Caucasic: ? Archi ba-sa `when?'; Andi e-bi `what?'
Burushic: (Y) bo, be `what?, how?', bé-sa `why?'
Sino-Tibetan: P-ST *Pa `what? which?'; Burmese ba
`id.'

39.WHO?$_2$ Sumerian lú
Na-Dene: Haida dluu `so then; when, if
conjunction)'; Chipewyan-dlá- (interrogative
stem, e.g., ʔɛdlãγĩ `who')

40.WIDE Sumerian tál
Burushic (H) daldal-um `wide'
Sino-Tibetan: P-ST *dhĕlH `wide; to spread, extend';
Tibetan r-dal `to spread, cover'
Na-Dene: Sarsi -táál `to be wide'; Navajo -tèèl, -
téél`id.'

41.WIND Sumerian líl `wind; to blow'
Basque luia `adverse wind'
Caucasic: P-NC *ɬwəəlʔV `wind, to blow'; Akhwakh ɬwē /
ɬoyi `wind, breeze', etc.
Sino-Tibetan: P-ST *lij = liy `wind, breeze'; Burmese
lij = liy `wind', etc.
Yeniseian: P-YEN *ʔuly- `whirlwind'
Na-Dene: Chipewyan -yùɬ , -yúɬ `to blow'; Navajo yóɬ ,
yòl, -yòɬ `to blow, be windy', etc.

ANALYSIS: Out of 41 comparisons, almost all pertain to the most basic lexical stratum, in terms of the 100-word and 200-word lists used in glottochronology and lexicostatistics (allowing for some simple semantic changes, since no calculation is being made here). In fact, of the semantic headings above (BLOOD, etc.), only four (BREAK, LIP, MAKE, SINEW) are not usually found on such lists. Yet it is obvious that even these latter meanings are basic, and might be part of a hypothetical 300- or 400-list. Ten of the semantic headings (BLOOD, DIE, FULL, HAND, I, NAME, NOT, WATER, WHO, SUN (in DAY) also pertain to the "Dolgopolsky list" of the 25 most stable meanings. This evidence indicates a strong correlation between the basic vocabularies of Sumerian (inasmuch as we can know it), and of the Dene-Caucasic languages. As far as I know, this close correlation cannot be matched by any other language (macro-)family.

<u>SUBGROUPING WITHIN DENE-CAUCASIC:</u> With disclaimers outlined below, Sumerian, in this set of comparisons, shows isoglosses (including the more doubtful cases marked "?") with individual Dene-Caucasic families, as follow:

Basque:	14	\
Caucasic:	15	| - - - - (Macro-Caucasic = 24) [21]
Burushic:	9	/
Sino-Tibetan:	23	
Yeniseian:	9	
Na-Dene	24	

(Cautionary notes: The sample of 41 comparisons (probably a fraction of the total possible corpus) is rather small for the purpose of deriving sweeping conclusions. The low numbers for some families [e.g., Burushic, Yeniseian] may be a result of their lack of internal diversity, less complete documentation [compared with other families], and, of course, my own unavoidable biases.)

Nonetheless, what impressed me about the numbers is that just as many isoglosses are found in the families most distant from the historical Sumerian homeland, namely <u>Sino-Tibetan</u> (23) and <u>Na-Dene</u> (24), as with the western Dene-Caucasic languages, taken together as <u>Macro-Caucasic</u> (24).

Some of the Sumerian-Na-Dene isoglosses are particularly striking, e.g., BLOOD (only Sumerian and Haida retain the original meaning); BREAST (Sumerian + Haida, again; see also EARTH, SKIN); COLD, DRINK, EARTH, FLY, LIVE (same meaning in Sumerian and Eyak); NOT, SKIN (transition to artifact

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[21] Editor's Note: It is not clear how he added this up but the total would be 38 were Burushic included, as it seems to be. Ruhlen's GUIDE lists 38 Caucasic, but 258 Sino-Tibetan and 34 Na-Dene languages. With Basque and Burushic counted as 1 each plus dialects, while Yeniseian is several languages, the `eastern' part of Dene-Caucasic should produce several times as many cognates or the proto-languages should reach farther into the past than the `western' part.

`wineskin' in Caucasic); STONE, WHO$_2$. I do not interpret these isoglosses as shared innovations, indicating that Sumerian, Sino-Tibetan, and Na-Dene form a subgroup of Dene-Caucasic. Rather, I think they are shared retentions (archaisms), evidence that Sumerian represents the only recorded remnant of a distinct Dene-Caucasic sub-group (ancient dialect), certainly no closer to what I call Macro-Caucasic or Vasco-Caucasic (Basque + Caucasic + Burushic) than to the far-flung Sino-Tibetan, Yeniseian, and Na-Dene. Similarly, the few Sumerian-Haida parallels do not indicate any special relationship between these languages (beyond a common Dene-Caucasic heritage), but simply archaic residue retained by the oldest recorded language (Sumerian), and the most divergent member of Na-Dene (Haida).

NOTES ON SUMERIAN MORPHOLOGY: The following observations on Sumerian and Dene-Caucasic morphology are largely exploratory, and should be taken as suggestions for further study. For example, very little is known -- or can be known -- about Proto-Sino-Tibetan morphology, since the modern languages are overwhelmingly of the isolating type, and rather sporadic traces of prefixes and suffixes are found, to a greater or lesser degree, in the attested languages. Classical Tibetan preserves the affixes particularly well, and is convenient to use in comparisons, though we must note that we do not really know whether Tibetan completely reflects the P-ST stage. Thus the uncertainty of these attempts.

When confronted by structural parallels such as these (bearing witness to a separable element s-, remaining in historical languages only as a "fossil"):

a.	Sino-Tibetan:	Tibetan	s-gal-pa	`small of the back'
	Na-Dene	Haida	s-qál	`shoulder'
b.		Tibetan	s-ku	`body'
		Haida	s-ku	`back'
c.		Tibetan	s-ked-pa	`waist'
		Haida	s-q'ut	`armpit'
d.		Balti	s-kil	`center'
		Haida	s-gíl	`navel'

Edward Sapir declared that (Classical) Tibetan "is startlingly Nadene-like" (letter to A.L.Kroeber, 1921; quoted by Bengtson, 1994).

I got a similar impression ("Sumerian is startlingly Na-Dene/Sino-Tibetan-like") when I read Thomsen's (1984) description of the Sumerian verb:

MODAL PREFIX + CONJUGATION PREFIX + CASE PREFIX + PRONOMINAL + (VERB STEM)

Compare the model reconstructed for Na-Dene (Krauss 1965)

MODAL + ASPECT + PRONOUN + CLASSIFIER + STEM

Both languages also have suffix positions. This type of struct-ure, with multiple prefix positions (both modal and pronominal) before the verb stem, is virtually unknown in Nostratic languages (except, perhaps, recently, under area influence of non-Nostratic languages), yet it is typical of several Dene-Caucasic languages, e.g., Basque:

n-	a-	kar-	k	`you (sg.) (are)
1st +	pres. +	(verb +	2nd sg.	carry(ing) me
sg.	tense	stem)	intimate	
abs.			male erg.	
d-	a-	kar-	t	`I (am) carry(ing)
3rd	pres.	(verb	1st	him'
pers.	tense	stem)	pers.	
			erg.	

d-	a-	kar-	ki-	o-	t	`I carry it to him'
3rd	pres.	verb	FLAG	3rd	1st	
pers	tense	stem		sg.	pers.	
				dative	erg.	

(Thanks to R.L.Trask for explanation of the verb forms.)

Note the presence of pronominal elements both before and after the verb stem in Basque, as in Sumerian (see Bomhard's article, this volume).

Structure is, of course, only typological, but if languages that share a significant number of basic lexemes also share typological features, the latter may constitute additional evidence of genetic unity. The clincher would be to establish sound-meaning parallels between Sumerian morphemes and Dene-Caucasic morphemes. I suggest this as a potentially rich field for future research.

NOTES ON MORPHOPHONEMIC ALTERNATION (ablaut, stem variants): In the lexical comparisons above, we have seen some cases where a Sumerian word has two or more recorded variants, e.g., Sumerian `to live' (§20, above):
tìl ti-la tin ti `to live (be alive)' (as cited by
Boisson,1989b)

I find these variant highly reminiscent of some Na-Dene stem alterna-tions, e.g., (in Sarsi):

-tàh -tí(n) -tâɬ `to handle a living being';

In Sarsi (and other Na-Dene languages) the stem variants are associated with different aspects (imperfective, perfective, etc.) of the verb. Note the alternation of **n ~ l ~ 0/H** in both languages. The last alternant suggests a change from vowel + nasal [VN] -> nasalized vowel [V~, e.g.,ã ẽ] -> plain vowel [V]. In the next example, an original alternation in Sumerian of vowel + nasal with nasalized vowel may be a possible explanation:

§8: Sumerian nag̃ , na$_8$ \`to drink' (phonetic [na$_{\eta}$ ~ nã] ?)
Cf. Na-Dene: Mattole -naah, -naan, -naaʔn, -naal, -na$_{\eta}$ʔ \`to drink'; Tlingit náá, ná \`to drink'

Cf. also the alternation in Sumerian te ~ ten \`(be) cold' (§5, above). Stem alternation of this type, VN ~ V~ ~ V, is frequent in Na-Dene, and, perhaps surprisingly, in Burushic:

Na-Dene: Tlingit -sàà (pret.), -sáá (fut.), vs. -sén (inceptive)\`to name'
Cf. Burushic -sí (durative), vs. -sén (non-durative) \`to say, to name); Sumerian sa$_4$ \`to name, to call by name'(phonetic **[sã]** ?); Basque izen \`name' (= §27, above)

Burushic -γán-, -γái-, (n↗-)γan, (ni-)qá \`to look, see'; γen \`seen, visible', a-qén \`unseen, invisible'.
Cf. Na-Dene: Tlingit -Gèn, -Gén, -Gɛ́n \`to look'
Sino-Tibetan: P-ST *qē̜n \`to see, look, know';
Sumerian kin, kig̃ \`to seek', etc.

What we see here are probably the results of independent processes of vowel nasalization, thence (in some cases) loss of nasality **VN > V~ > V** in certain verbal forms, while in others the original vowel + nasal remained VN, usually "protected" by a suffix.

Above we have already seen examples of ablaut (vowel altern-ation) in Na-Dene and Burushic. In Sino-Tibetan, ablaut is strongly evident in Tibetan, e.g.:

Tibetan sem(s) (pres.), sem-s, b-sam-s (perf.), b-sam (fut.), som (imperative) \`to think';

cf. Na-Dene: Hupa -si$_{\eta}$, -si$_{\eta}$ʔ , -sin, -seʔn \`to feel, think'

Ablaut in both Sino-Tibetan and Na-Dene was one of the "Sino-Dene" typological parallels noted by Edward Sapir. Compare also the alternations below (lexicalized in Tibetan):

Tibetan r-dog \`to kick', vs r-dug \`to conquer, vanquish, destroy; to strike against';
cf, Na-Dene: Tlingit (inceptive) t'úkw, vs. (preterit) t'ùkw,(fut.) t'úkw \`to shoot with an arrow';
Chipewyan -t'òγ, -t'òih (versus) -t'ù \`to sting'
cf. Sumerian dug$_4$-ga \`to strike, beat, hit, smite, kill'.[22]

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[22] Editor's Note: The reading on the Sumerian form may be [duq$_4$-qa], i.e., a [q] rather than a [g]. If fault be found, it lies with this editor's eyesight!

With possible lexicalized vowel alternations in Sumerian, Cf (§36):

> Sumerian lux `to wash' ~ (reflexive) làx `to wash oneself'.
>
> Cf. Na-Dene: Chipewyan lɛ́ (perfective), -lɛ̀ (future), -làį (progressive)

I suggest the Sumerologists work with us to determine whether or not the apparent Sumerian stem variants may have originated in the same way as the alternations observed in Dene-Caucasic languages. If so, these and other morphological patterns may reflect what Sapir termed "linguistic drift", i.e., parallel developments in related languages after their dispersal and geographic separation.

PROPOSALS BY DIAKONOFF AND BOMHARD: Diakonoff's proposal (comparing Sumerian with Munda) is very interesting, and some of his comparisons are plausible, especially, viz., Sumerian **-ak** vs Kherwari **-a(k')** `genitive marker'; **ene** vs **en-** `3rd person pronoun'; **gaz** vs **gač** `to kill'; **ŋa(e)** vs **(a)iŋ** `I'; **xa** vs **ha-** `fish'; **ŋi(g)** vs **ŋinda** `night'; **-ta** vs **-ta** `ablative'. On the other hand, of the 34 comparisons, only 13 involve meanings from the 200-word list, viz., father, mother, wash, big, kill, I, my, fish, thy, night, foot, name, dog. Of these five (I, my, thy, night, name) fall within the Dolgopolsky list. (See above for the corresponding figures in the Sumerian-Dene-Caucasic comparisons.)

Some of Diakonoff's comparisons are inherently plausible, but can be matched or surpassed by Dene-Caucasic comparisons, such as Sumerian **aba** (etc.) and Munda **apa** `father' (cf. Caucasic **\*ʔŏbVV-** `father', Yeniseian ***ob**, etc.; and Nostratic ***ʔab-**, cited by Bomhard); Sumerian **ag** `make, do' and Munda **agu-** `bring' (the meanings are more similar in the Caucasic comparison, §23, above); Sumerian **ama** and Munda **uma** `mother' (cf. Basque **ama**, Nostratic **\*ʔam(m)-**, etc.); Sumerian **ene**, **ane** and Munda **en-** (3rd person; cf. Burushic **in**, **iné** `id.'; and Nostratic parallels cited by Bomhard); Sumerian **ŋa(e)** and Munda ***Vŋ** `I' (P-ST **\*ŋ ā** is more similar); Sumerian **mu** // **ŋu**$_{10}$ `name' and Munda **num** (cf. Dimasa **mu**, §26, above). Words such as `mother' and `father' are so universal as to be virtually useless for classification (unless there is a distinctive feature, such as the element ***-ter-** in the Indo-European words).[23] The word for `name' cited by Diakonoff (Santali **num**) is also found in several macro-families (cf. English `name' **[neym]**, Finnish **nimi**, Yukaghir **niu**, etc.), but the metathesis of the type ***mVŋ-** (as in Sino-Tibetan, and, as I propose, in Sumerian) is distinctively Dene-Caucasic.

Otherwise, some of Diakonoff's comparisons assume very dubious semantic shifts, e.g., \`wish, curse' = \`bring, fetch, get'; \`cripple, dwarf' = \`rejected, refuse'; \`obsessed' = \`idiot'; \`fertile' = \`wet'; these cannot be taken seriously as evidence for genetic connection. All in all, I do not believe that Diakonoff has demonstrated a specific genetic connection between Sumerian and the Munda languages. The relatively few plausible comparisons can be explained either as chance resemblances, or as archaic residue from a very early stage -- Proto-Human, or perhaps somewhat later in the dispersal of humanity. In other words, it is not that Sumerian and Munda are "unrelated", but that the relationship is probably indirect, by virtue of the common origin of the Dene-Caucasic and Austric macro-families, and too remote to shed any light on the immediate origins of the Sumerians.

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[23] Editor's Note: Bengtson presumably means that the specific **baba/mama** terms for father and mother are so common as to be useless for sub-classification. But not that the meanings `father' and `mother' necessarily take the same forms universally.

Bomhard's article is also very interesting. His conclusion is (rightly, I think) very restrained, viz. that Sumerian is related to Nostratic, but not a "daughter language" of Nostratic. I think a significant number of his grammatical and lexical comparisons are probably valid, but reflect the deeper level of relationship proposed in Starostin's (1989) "Nostratic and Sino-Caucasian", or Fleming's (1988) "Borean". Bomhard presents quite a large number of lexical comparisons (I counted 139), but note that the great majority of them involve verbal stems, and only about a dozen (e.g., I, who/what, seed, wide, say, blood, wash, not, breathe, blow, breast) pertain to the basic core of meanings represented by the 200-word list.[24]

As in Diakonoff's list, some of Bomhard's Sumerian-Nostratic comparisons can be matched by Dene-Caucasic comparisons, e.g., Sumerian gú \`head, forehead' (cf. P-ST **\*qhōwH** \`head'; Tibetan **m-go**, etc.); Sumerian **gu-ru-un** \`blood' (cf. §1. BLOOD, above); Sumerian **xu-ri-in** \`eagle' (cf. Basque **arrano**; P-EC ***ʔār-[c̱'wämʔV]** \`eagle'); Sumerian **làx** and **lux** \`wash' (cf. §36. WASH, above); Sumerian **tab** \`burn' (cf. §4. BURN, above); Sumerian **tum** \`abundance' (cf. P-ST ***dh[ia]mH** \`full, fill'; Tibetan **g-tam**, **g-tom**, etc.).

CONCLUSIONS: My re-examination of the lexical evidence, together with recent discoveries in Dene-Caucasic morphology, re-affirm my original hypothesis (above), that Sumerian was most likely a Dene-Caucasian language. Based on the analysis of basic vocabulary, I conclude that Sumerian represents the remnant of a distinct Dene-Caucasic subgroup, coordinate with the other branches, as detailed above.

ABBREVIATIONS

H	Hunza (dialect of Burushic, née Burushaski)
P-EC	Proto-East-Caucasic (Nikolaev & Starostin)
P-NC	Proto-North-Caucasic (Nikolaev & Starostin)
P-ND	Proto-Na-Dene (usually Pinnow)
P-ST	Proto-Sino-Tibetan (Peiros & Starostin)
P-YEN	Proto-Yeniseian (Starostin)
Y	Yasin (= "Werchikwar" dialect of Burushic, née Burushaski)

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[24] Editor's Note: Before someone demands a recount, we can witness that there are about two dozen meanings from the Swadesh 200-list on the Sumerian side of the ledger.

// Eme-gir // Eme-sal (varieties of Sumerian)

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[25] Editor's Note: An older, perhaps preliminary, version of this hypothesis was given in 1984 at the famous Moscow conference. It is likely that Starostin was at least aware of Swadesh's "Vasco-Dene" at that time, since his teacher, Dolgopolsky, had been in touch with Swadesh.

MOTHER TONGUE

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ON THE ORIGIN OF SUMERIAN

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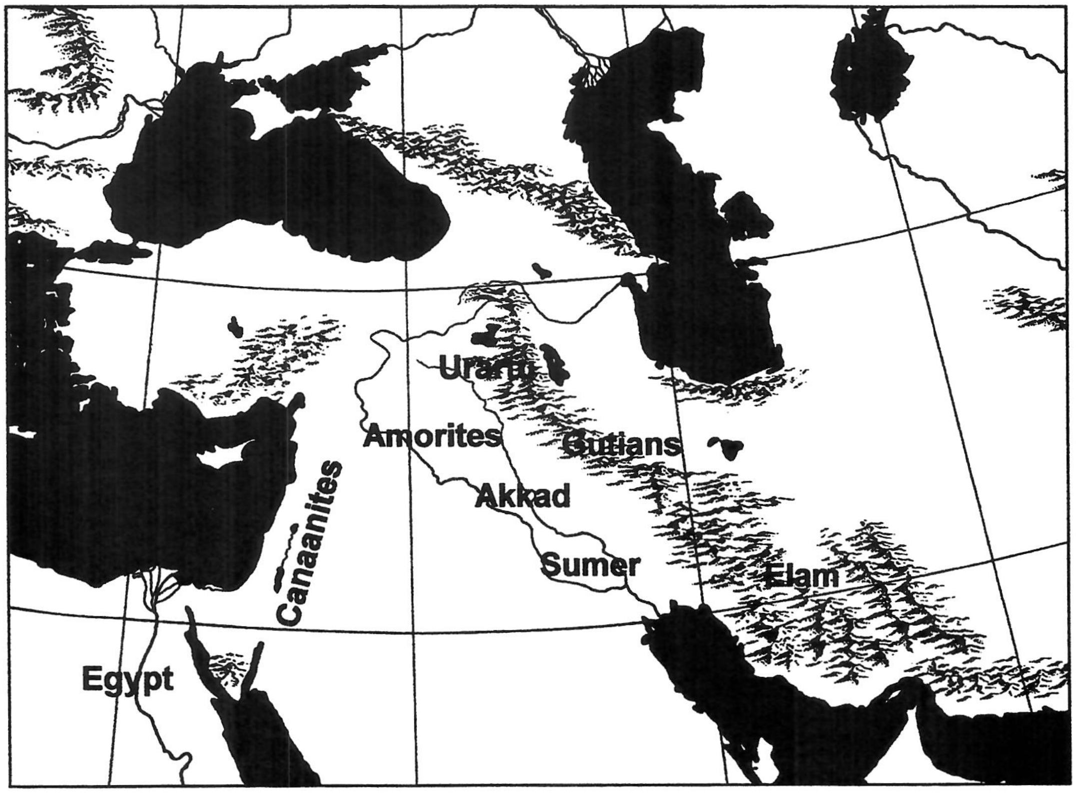
1. Introduction

Sumerian, which is now extinct, was spoken in southern Mesopotamia (modern-day Iraq), extending from Babylon in its northernmost limits to the tip of the Persian Gulf in the south (see map below). From the time of the earliest texts, several dialects can be distinguished, the most important of which was Emesal, most probably "women's speech", which Boisson (1992:434—435) argues was more conservative than the main dialect, Emegir. The earliest Sumerian inscriptions date from around 3,100 BCE, though the oldest intelligible texts date from about 2,600 BCE, and the language was probably still spoken as late as the 3rd century BCE. The Sumerian writing system was based exclusively on the cuneiform syllabary, which exhibits several marked stages of development over the course of Sumerian literary history. After about 1,900 BCE, Akkadian (a Semitic language) began to replace Sumerian in letters and administrative texts, though Sumerian continued to be used in cultic and literary texts.



Sumerian shares a number of interesting lexical parallels with the Nostratic languages (these are listed and discussed in several papers by Boisson and in Bomhard—Kerns 1994:195—714 — they are summarized in an appendix at the end of this paper), including some core vocabulary items such as pronominal stems, though there are important differences here as well. Thus, Sumerian *may* in some way be related to the Nostratic languages. In a number of privately-circulated papers, Claude Boisson has explored lexical parallels between Sumerian and Dravidian, while Anumugam Sathasivam (1965), in an unpublished manuscript, has tried to show that Sumerian is related to Dravidian. Though I formerly very tentatively accepted a modified version of Sathasivam's (and Boisson's) theories, placing Sumerian as a sister to Proto-Elamo-Dravidian, I am not entirely satisfied with this arrangement. True enough, Sumerian has an agglutinating morphological structure, as do Elamite and Dravidian, and the nominal case endings, for example, are, in reality, bound postpositions in both Sumerian and Elamo-Dravidian. However, Sumerian is sufficiently different from both Elamite and Dravidian to make me question that there was a special relationship between them.

2. Notes of Sumerian Morphology

Before beginning, we should give a brief sketch of Sumerian grammatical structure, noting first and foremost that, even after more than a century of intensive study, there is still not widespread agreement among experts in the field on many fundamental questions of Sumerian grammar. Nevertheless, the overall structure is clear. Three word classes were distinguished: (A) nouns, (B) verbs, and (C) adjectives. Even though grammatical gender in the strictest sense did not exist, nouns fell into two classes, namely, animate and inanimate, which were only distinguished in the 3rd person actor verbal and possessive pronoun affixes and in the relative pronoun. Ten cases (genitive, absolutive, ergative, dative, locative, comitative, terminative, ablative-instrumental, and equative [in nouns] plus subject case [in pronouns only]) and two numbers (singular and plural) were distinguished. The plural was indicated by means of the suffix *-ene*, which was used only with animate nouns, or by reduplication. In later texts, the plural could also be indicated by the form *hi-a*, which was used with inanimate nouns and which was originally an independent word meaning 'mixed, various, unspecified', or by *-me-eš*, which was properly the enclitic copula with plural suffix. Sumerian differentiated between ergative and absolutive in nouns. In pronouns, however, the patterning was that of a nominative-accusative system (so Thomsen 1984:51, §42, and Michalowski 1992:96; Diakonoff, however, disputes this [personal communication]). Sumerian verbs were formed by adding various prefixes and/or affixes directly to the verbal root, which was itself invariable. Verbal constructions fell into one of two categories, namely, finite forms or non-finite forms. Finite verbal stems distinguished three conjugational types: (A) the intransitive conjugation, (B) the transitive *hamṭu* conjugation, and (C) the transitive *marû* conjugation. Intransitive forms were noted by means of pronominal suffixes, while transitive forms were noted by means of either prefixes, suffixes, or both. Syntactically, the basic word order was SOV.

3. Sumerian Phonology

The Sumerian cuneiform syllabary distinguished the following sounds:

p	t		k	
b	d		g	
	s	š		h
	z			
m	n		g̃ (= [ŋ])	
	l			
	r			
*w		*y		
a	e	i	u	

There may have been corresponding long vowels as well. There were no initial consonant clusters, while final consonants, especially *t*, *d*, *k*, *g*, *m*, *n*, and *r*, were often omitted in the writing (cf. Thomsen 1984:43), and this often makes it difficult to ascertain the form of the word. Internally, there was a tendency for consonants to assimilate. The traditional transliteration shows a voicing contrast in stops. There is a very strong probability, however, that the actual contrast was between voiceless aspirated versus voiceless unaspirated or simply between tense versus lax (cf. Boisson 1988b:215—19; Thomsen 1984: 43): traditional *p*, *t*, *k* = p^h, t^h, k^h respectively, while traditional *b*, *d*, *g* = *p*, *t*, *k* respectively. Traditional *z* may have been an affricate (cf. Boisson 1989b:221—26). Though the semivowels /y/ and /w/ were not directly represented in the writing system, there is indirect orthographic evidence of their existence. Lastly, Boisson (1989b:212—214) considers Bauer's proposed d^r (cf. Thomsen 1984:44) to be highly questionable. For a discussion of the problems involved in interpreting Sumerian phonetics and phonology, cf. Diakonoff 1992:125—129.

The Sumerian root was generally monosyllabic: *CV*, *VC*, and, most often, *CVC*. There was no distinction between verbal roots and nominal roots — thus, for example, *dùg* could mean either 'good' or 'to be good'.

In the Sumerian texts, certain non-standard forms of speech can be discerned. It is not entirely clear what this means — perhaps different dialects, perhaps not; perhaps so-called "refined speech", perhaps not. These forms, which have been encountered mostly in religious texts, were labeled "Emesal" by the scribes, while the standard forms were labeled "Emegir".

4. Clues about the Origin of Sumerian

To illustrate the problems involved in trying to determine the origin of Sumerian, let us begin by looking at the differences between the case endings reconstructed for Proto-Elamo-Dravidian by McAlpin (1981:111) with those found in Sumerian (cf. Thomsen 1984:88—89):

A. Proto-Elamo-Dravidian:

Nominative:	*-Ø
Accusative:	*-(V)n
Adessive/	*-əkkə
Purposive (Dative):	(?)
Genitives:	
1. Possessive:	*-a
2. Adnominal:	*-in
3. Oblique/Locative:	*-tə

B. Sumerian:

	Postpositions / "case endings"		
Case	Animate	Inanimate	Prefix Chain
Genitive:	-ak	-ak	
Absolutive:	-Ø	-Ø	
Ergative:	-e	-e	
Dative ("to, for" — animate only):	-ra		-na-, etc.
Locative ("when"):		-a	-ni-
Comitative ("with"):	-da	-da	-da-
Terminative ("to"):	-šè	-šè	-ši-
Ablative ("from")-Instrumental:		-ta	-ta- and -ra-
Locative-Terminative:		-e	-ni-
Equative ("like, as"):	$-gin_7$	$-gin_7$	

The prefix chain cases require special explanation (I will quote from Thomsen 1984:215 and 219 [for the dative, §431 below]):

> **§ 423.** Some cases, the so-called dimensional cases, can be incorporated in the prefix chain of finite verbal forms. These cases are: dative, comitative, terminative, ablative, and locative. In principle the case elements have the same shape as the corresponding postpositions and only minor changes in writing and pronunciation occur.

> The rank of the case elements in the prefix chain is between the conjugation prefixes and the pronominal element serving as subject/object mark...
>
> **§ 424.** *Terminology*
>
> The case elements of the prefix chain are most often called 'infixes' or 'dimensional infixes' by the sumerologists. However, since they do not act as infixes in the stem but merely as members of the chain of grammatical elements preceding a verbal root, 'case elements' or 'case prefixes' are used here as the most appropriate terms.
>
> **§ 431.** The dative is the only case prefix which has different prefixes for every person...

1.sg.	ma- < /mu-a-/	1.pl.	-me-
2.sg.	-ra-	2.pl.	?
3.sg.an.	-na- < /-n-a-/	3.pl.	-ne-

There are parallels, to be sure, but as many with *other* Nostratic languages as with Elamo-Dravidian. The Sumerian ablative-instrumental case ending (inanimate) *-ta*, (prefix chain) *-ta-* agrees with the Proto-Uralic ablative ending **-ta* as well as with the Proto-Elamo-Dravidian oblique/locative ending **-tǝ* The Sumerian locative case ending (prefix chain) *-ni-* is similar to the Proto-Uralic locative case ending **-na*, though the vowels are problematic, and to the Proto-Dravidian locative case ending **-in*(/**-il* ?). The Sumerian genitive case ending -*ak* is similar in form to the Proto-Dravidian dative case ending **-(k)ku* and the Proto-Elamo-Dravidian adessive/locative (dative) **-ǝkkạ* but the difference in function is a problem. Moreover, the *-na-* and *-ni-* prefix chain case endings may be somehow related to the oblique-*n* formations described by John C. Kerns (cf. Bomhard—Kerns 1994:173—179, §3.5.3.1).

An extremely interesting parallel involves the Sumerian comitative element *da* (also -*dè*). As noted by Thomsen (1984:99): "The basic meaning of the comitative is 'with', 'together with', expressing accompaniment as well as mutual action." A particle **da/*dạ* with the basic meaning 'along with, together with, in addition to', shows up all over Nostratic (cf. Bomhard—Kerns 1994:275—276, no. 89). It appears in Kartvelian as a conjunction: Georgian *da* 'and', Mingrelian *do* 'and', Zan *do* 'and' < Proto-Kartvelian **da* 'and', and probably as the adverbial case ending *-ad/d* found, for example, in Old Georgian (in Modern Georgian, the ending is *-ad[a]*). In Afrasian, it is found in Chadic: Hausa *dà* 'with; and; by, by means of; regarding, with respect to, in relation to; at, in, during; than'; Kulere *tu*; Bade *dạ* Tera *ndạ* Gidar *di*; Mokulu *ti*; Kanakuru *dǝ* < Proto-Chadic **dǝ* 'with, and'. It may also occur in Cushitic in the Burji locative suffix *–ddi*, as in *mináddi* 'in the house'. Elamite has *da* 'also, too, as well, likewise; so, therefore, consequently, accordingly, hence; thereby, thereupon'. Particularly interesting is Altaic, where this particle functions as a locative suffix on the one hand, **-da*, and as an independent particle on the other, **da* 'together with, and, also': Common Mongolian dative-locative suffix **-da* > Mongolian *-da*; Dagur *-da*; Khalkha -*dɒ*; Buriat *-da*; Kalmyk *-dɒ* (cf. Poppe 1955:195—199).). In Manchu, the dative-locative particle is *-de*. In Turkic, it also appears as a locative suffix: Common Turkic **-da/*-dä* (cf.

Menges 1968:110). It may be preserved in Indo-European in the suffixed particle appearing, for example, in Sanskrit as *-ha* and *-dhi*: *sa-há* 'with' (Vedic *sa-dha*), *i-há* 'here' (Prakrit *i-dha*), *kú-ha* 'where?', *á-dhi* 'above, over, from, in'; in Avestan in *iδa* 'here', *kudā* 'where?'; and in Greek in the locative particle -θι in, for example, οἶκο-θι 'at home', πό-θι 'where?'.

Now let us look briefly at verb morphology. McAlpin (1981:122—123) notes that the Proto-Elamo-Dravidian verbal conjugation "does not survive in Dravidian as a paradigm". Therefore, we will give the verbal endings as they appear in Middle Elamite, using, once again, the verb *hutta-* 'to make' for illustration (cf. Reiner 1969:76; Grillot-Susini 1987:33):

Person	Singular	Plural
1	hutta-**h**	hutta-**hu** (< h + h)
2	hutta-**t**	hutta-**ht** (< h + t)
3	hutta-**š**	hutta-**hš** (< h + š)

McAlpin derives the Elamite 1st sg. ending *-h* from Proto-Elamo-Dravidian **-H*, the 2nd sg. ending *-t* from **-ti*, and the 3rd sg. ending *-š* from **-(V)š*. The Proto-Elamo-Dravidian 2nd sg. ending **-ti* survives in South Dravidian negative imperatives.

The Sumerian finite verb employs various pronominal elements. These are described by Thomsen (1984:147, §287) as follows:

> The pronominal elements of the finite verbal form refer to the persons involved in the verbal action. There are two main series with different marks: the prefixes and the suffixes. A verbal form can have at most one prefix immediately before the verbal root and one suffix after the verbal root (or, if present, after /ed/), both referring to subject and/or object. The prefixes are identical with the pronominal elements which under some conditions occur together with case prefixes...

Thomsen (1984:148—149, §290) lists the following pronominal prefixes:

1.sg.	-?-	1.pl.	-me-
2.sg.	-e-	2.pl.	-e ene-
3.sg. animate	-n-	3.pl.	-ene-
inanimate	-b-		

The plural pronominal prefixes "are used as dative elements only..., and it is thus more probable that they are case elements rather than pronominal elements" (cf. Thomsen 1984:148).

The Sumerian pronominal prefixes are strongly reminiscent of the possessive suffixes/personal endings found in various Nostratic daughter languages — note, for example, the Proto-Uralic personal endings, which have been reconstructed as follows (cf. Hajdú 1972:40 and 43—45; Sinor 1988:725):

Person	Singular	Plural
1	*-me	*-me (+ Plural)
2	*-te	*-te (+ Plural)
3	*-se	*-se (+ Plural)

Even more interesting are the possessive suffixes reconstructed for Proto-Tungus (cf. Sinor 1988:725):

Person	Singular	Plural
1	*-m	*-m (+ Plural) (exclusive)
2	*-t	*-t
3	*-n	*-t

Similar forms are found in Indo-European, Kartvelian, and Afrasian. The first person possessive suffixes/personal endings in **-m* found in various Nostratic daughter languages are similar in both form and function to the Sumerian first person pronominal prefixes, 1st singular *ma-* (< /mu-a-/) and 1st plural *–me-*, while the Proto-Tungus third person singular possessive suffix in **-n* (related forms are found in other Nostratic daughter languages) is similar to the Sumerian third person pronominal prefixes, 3rd singular *–n-*, *-na-* (< /-n-a-/) and 3rd plural *–ne-*, *-ene-*.

There are also two series of pronominal suffixes (cf. Thomsen 1984:152), the first of which (column A below) marks both the subject of intransitive verbs and the direct object of transitive verbs, the second of which (column B below) "is used in two-part. *marû* forms together with the prefix /-n-/ to denote the 3.pl. ergative subject". In actual fact, only the 3rd persons singular and plural are different (cf. Thomsen 1984:152).

	A		B	
	sg.	pl.	sg.	pl.
1	-en	-enden	-en	-enden
2	-en	-enzen	-en	-enzen
3	-Ø	-eš	-e	-ene

There is simply nothing here that resembles what is found in Elamo-Dravidian nor, for that matter, at least for the first and second persons singular and plural, in other Nostratic languages. The third person pronominal suffixes, however, do have parallels in various Nostratic daughter languages. For a discussion of the etymology of the pronominal stems, see below.

The Sumerian personal pronoun stems are as follows (the Emesal forms are shown in parentheses; /g̃ / = /ŋ/)(cf. Thomsen 1984:68; Boisson 1992:437):

	1.sg.	2.sg.	3.sg.	3.pl.
Subject:	g̃ á.e	za.e	e.ne	e.ne.ne
	(me.e)	(ze)		
	g̃ á-a-ra	za-a-ra	e.ne-ra	e.ne.ne-ra
Dative:	g̃ á-a-ar	za-a-ar		
	(ma-a-ra)			

Terminative:	g̃ á(-a/e)-šè	za(-a/e)-šè	e.ne-šè	e.ne.ne-šè
Comitative:	g̃ á(-a/e)-da	za(-a/e)-da	e.ne-da	e.ne.ne-da
Equative	g̃ á(-a/e)-gin$_7$	za(-a/e)-gin$_7$	e.ne-gin$_7$	e.ne.ne-gin$_7$

The possessive suffixes are (cf. Thomsen 1984:71):

	Singular	Plural
1	*-g̃ u$_{10}$* 'my'	*-me* 'our'
2	*-zu* 'your'	*-zu.ne.ne*, *-zu.e.ne.ne*, *-zu.ne* 'your'
3 an.	*-a.ni* 'his, her'	*-a.ne.ne* 'their'
inan.	*-bi* 'its'	*-bi* also 'their', presumably collective

Right away, we notice that the Emesal 1st singular forms (subject) *me.e*, (dative) *ma-a-ra* are related to the common Nostratic 1st person personal pronoun stem **mi*/**me* 'I, me' (cf. Bomhard—Kerns 1984:661—662, no. 540; Illič-Svityč 1971— .II:63—66, no. 299 **mi*), while the 1st plural possessive suffix *-me* is related to the common Nostratic inclusive 1st plural personal pronoun stem **ma-*/**mə* 'we, us' (cf. Bomhard—Kerns 1984:661—662, no. 540; Illič-Svityč 1971— .II:52—56, no. 289 **mä*). The 2nd person personal pronoun *ze-*, *za-*, *-zu* may also be derived from the Proto-Nostratic 2nd person personal pronoun stem **tʰi-*/**tʰe-* 'you' (cf. Bomhard—Kerns 1984:285—287, no. 102; Dolgopolsky 1984:87—89 **t̯[ü]*), assuming affricatization of the dental before front vowel (similar to what has happened in Mongolian): **tʰi-*/**tʰe-* > **tʸi-*/**tʸe-* > (**tˢi-*/)**tˢe-* > *ze-* /t͜se-/, etc. (Sumerian <z> = /t͜s/ [cf. Boisson 1989:221—226 and 1992:436]). Finally, the 3rd person forms *e.ne* and *a.ne* are related to the demonstrative pronoun *ne.en*, *ne(-e)*, which has a parallel in the Proto-Nostratic demonstrative stem **na-*/**n‹-*, **ni-*/**ne-*, **nu-*/**no-* (cf. Bomhard—Kerns 1984:688—689, no. 570). To account for the beginning vowels in *e.ne* and *a.ne*, Shevoroshkin (cited in Boisson 1992:443) has suggested that these appear "to be a compound of the demonstrative / personal pronoun of the 3rd person ***#i* / ***#ä* [...] plus the demonstrative base ***n(ä)*". I agree with Shevoroshkin's suggestion. Though widespread in the Nostratic daughter languages, these stems are lacking in Dravidian (though see Dolgopolsky 1984 for a slightly different interpretation of some of the Dravidian material). Zvelebil (1977:40) reconstructs the following personal pronoun stems for Proto-Dravidian:

	Singular	Plural
1	**yān* : **yan-* 'I'	(incl.) **yām* : **yam-* 'we' (excl.) **nXm* : **nam-* 'we'
2	**nīn* : **nin-* 'you'	**nīm* : **nim-* 'you'
3	**tān* : **tan-* 'he, she, it'	**tām* : **tam-* 'they'

McAlpin (1981:112) begins his discussion of pronouns by making some very important observations regarding the relationship of the Elamite and Dravidian pronouns:

> 530.0 The personal pronouns have long been an enigma in the relationship of Elamite to Dravidian. On the one hand, the second person pronouns provided the morphological detail first recognized as being cognate... On the other hand, one of them, the first person plural is still somewhat ambiguous as to its form in PED. For the others, it has been a long quest, fitting together the morphological pieces. The major breakthrough came with the realization that the Proto-Dravidian pronouns were not ultimately archaic, but rather a major innovation in late Pre-Dravidian. The nature of the innovation was the replacement of the nominative by oblique stems. Thus, Proto-Dravidian pronouns have little to say directly about the morphology of nominative bases in PED. However, the same forms, in a different usage, were preserved as personal possessive prefixes in kinship terminology. This was maintained as a system for a few kin terms in Old Tamil and sporadically in many other Dravidian languages. Thus, Dravidian does attest the PED system, but not directly in the paradigm.

McAlpin (1981:112—117) reconstructs the following personal pronoun stems for Proto-Elamo-Dravidian:

	Singular	Plural
1	*i	*nə̀NKə
2	*ni	*nim
3 resumptive	*ta(n)	
reflexive	*i	

The 1st person singular is to be derived from Proto-Nostratic **ʔiya* 1st person personal pronoun stem (postnominal possessive/preverbal agentive) found also in Afrasian (cf. Bomhard—Kerns 1984:597—598, no. 470; Dolgopolsky 1984:72, 83, 85—86, 96, and 99—100), while the 3rd person stem **ta(n)* is to be derived from the wide-spread Nostratic demonstrative stem **tʰa-*/**tʰə* 'this' (cf. Bomhard—Kerns 1984:287—289, no. 103), and the Proto-Dravidian 1st plural (exclusive) stem **nām* : **nam-* 'we' is to be derived from the Proto-Nostratic 1st person personal pronoun stem **na-*/**nə* (cf. Bomhard—Kerns 1984:683—684, no. 564; Dolgopolsky 1984:90—91) — this stem may also be the source of the Sumerian 1st person pronoun *g̃ á-* /ŋa-/, but this is uncertain.

5. Conclusions

The evidence surveyed in this paper indicates that Sumerian does not bear a special relationship to Elamo-Dravidian. Moreover, it does not appear to be a Nostratic daughter language in its own right either. Rather, the evidence seems to indicate that Sumerian is related to the Nostratic languages as a group, that is to say that it is a relative of Nostratic. However, there are also many problems that must still be solved regarding the exact nature of that relationship — we have only scratched the surface in this brief summary.

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Lexical Parallels Between Sumerian and Proto-Nostratic
(as reconstructed by Allan R. Bomhard)

Sumerian	**Meaning**	**Proto-Nostratic**	**Meaning**
a-aA	I	?a-/?ə	1st singular pronoun stem
a-ba, ab, ab-ba	Father	?ab-	father
a-ba, a-na (?)	who?, what?	?ay-, ?ya-	relative ~ interrogative pronoun
AK	to strike	?ak^{h}-/?ək^{h}-	to cut, to strike, to wound, to hurt
al-è	to light up, to shine, to brighten up	hal-/həl-	to light up, to beam forth, to shine
ama	Mother	?am(m)- /?əm(m)-	mother
aš-te, eš-de	seat, stool, throne	?as^{y}-/?əs^{y}-	to put, to place, to set; to sit
ba	to give as a gift or ration	bay-/bəy-	to apportion, to divide into shares
ba-ba-a	old man	baba	father
bad-du, bad$_{5}$	to separate, to divide, to part	bad-/bəd-	to split, to cleave, to separate, to divide
bad$_{4}$	need, difficult situation	bad-/bəd-	need, want, lack, deprivation
bal	to pour out, to overflow, to spill	bal-/bəl-	to well up, to surge, to overflow
bar	origin, descent, ancestry; family	bar-/bər-	to bear children, to give birth
bar	Seed	bar-/bər-	seed, grain
bar	to shine, to light, to illuminate, to glisten	bar-/bər-	to shine, to be bright, to sparkle
bar	to split (with a tool or a weapon), to cut	bar-/bər-	to cut, to cut off, to cut down; to carve
bar	open land, steppe, wasteland, desert	bar^{y}-/bər^{y}-	to be or become barren, desolate
bàr, bàra, bara$_{4}$	to spread or stretch out	bar-/bər-	to swell, to puff up, to expand
bar, bar$_{7}$	to blow (at or upon)	bar-/bər-	to blow
bi	with, together with, in addition to	bi/be	in addition to, with, together with
bùr	to bore through, to pierce	bur-/bor-	to bore, to pierce
bún	to blow, to inflate	bun-/bon-	to puff up, to inflate, to expand, to swell
		bung-/bong-	to puff up, to inflate, to expand, to swell
BU	to reach or arrive at a destination	buw-/bow-	to go, to come, to proceed
bu-bu-ul	boil, abcess	bul-bul-/bol-bol-	to swell, to bubble up

bu-i	Knowldege, learning	baw-/bəw-	to be or become aware of, to notice
bul	to blow, to breathe, to puff	bul-/bol-	to swell, to expand; to puff up, to inflate
bulug̃ $_7$	to grow, to make grow	bul-uʕ-/bol-uʕ-	to ripen, to bloom, to sprout, to mature
bur	to spread out, to cover over	bur-/bor-	to cover, to enclose, to wrap up
da	with, together with, along with, besides	da/də	along with, together with, in addition to
dab	to grasp, to seize, to take; to fasten	dab-/dəb-	to join or fit together, to fasten
dadag, dág	shining, bright, clear	daɢ-/dəɢ-	to glitter, to shine, to burn brightly
dag	residence, dwelling place	dag-/dəg-	to put, to place, to be firmly established
dála	thorn, needle	dal-/dəl-	to cut, to prick, to piece, to gash
dalla	to widen, to stretch, to extend	t'al-/t'əl-	to stretch, to extend
dar	to split	t'ar-/t'ər-	to tear, to rend, to chop, to sever
dára	a band	dar-/dər-	to bend, to twist, to turn
dar$_4$, dara$_4$	dark	dar-/dər-	to be dark
dé, dè, di$_5$	to sparkle, to shine, to flare up	t'ay-/t'əy-	to shine, to gleam, to be bright
dé, de$_6$, di	to bring	day-/dəy-	to take, to bring, to convey
dig	to converse, to speak	diɢ-/deɢ-	to be confused, puzzled, perplexed; to speak in a confused manner, to murmur
dih	to press, to push; slab for molding clay	diqʰ-/deqʰ-	to crush, to pound, to mold or knead
dil-bad	to shine, to be radiant, to gleam	dilʸ-/delʸ-	to shine, to be or become bright
dím	to make, to fashion, to create, to build	t'im-/t'em-	to make, to fashion, to create
dim; dim-ma	band; to tie together, to fasten	t͜ɬ'im-/t͜ɬ'em-	to join, bind, or unite together
dù	to do, to make, to build, to set up	daw-/dəw-	to put, to place, to set; to set up
du, du$_8$, duh	to go, to leave, to depart, to go away	t'aw-/t'əw-	to leave, to go away; to send forth
du$_{12}$	to play (an instrument), to sing	daw-/dəw-	to sound, to resound, to make a noise
du-du-ru	high (mountain)	dud-/dod-	tip, point
dug$_4$	to say, to speak, to tell	t'uq'-/t'oq'-	to say, to speak, to tell
dug$_4$-ga	to strike, to beat, to hit, to smite, to kill	t'uk'-/t'ok'-	to knock, to beat, to pound
dul	to cover	t'ʸul-/t'ʸol-	to overshadow, to cover over
dun	to dig (with a hoe)	dunʸ-/donʸ-	to cut off, to cleave, to split
dungu	cloud	dunk'ʷ-/donk'ʷ-	to cover over, to obscure, to make dark
dùr	young animal	dur-/dor-	goat, sheep, ram
du$_7$	to butt, to gore	t'aw-/t'əw-	to hit, to strike
du$_9$	to run, to wander or roam about	dʸaw-/dʸəw-	to run, to flow, to gush forth
du$_7$; du$_8$	to impale; to split, to destroy	dʸaw-/dʸəw-	to split, to prick, to piece

è	to go out, to come out, to leave	ʔay-/ʔəy-	to come, to go
e	hither, here	ʔi-/ʔe-	adverbial particle: to, toward, near
en; en-na	as far as, up to; to, towards, near	ʔanʸ-/ʔənʸ-	to, towards, over, for, against, on
gal$_4$, gal$_4$la	vulva	q'alʸ-/q'əlʸ-	sexual organs, genitals
gan	to bear, to bring forth, to give birth to	k'an-/k'ən-	to get, to acquire, to possess
gan	to complete, to finish	k'an-/k'ən-	to complete, to finish
gán, gána	field, land, country	q'an-/q'ən-	field, land, (open) country
garadin, kàradin	bundle, sheaf	k'ar-/k'ər-	to twist, to turn, to bend, to wind
gen; ginna	small; child	gin-/gen-	to be young, small, weak
gír	girdle	gir-/ger-	to enclose, to gird
gú	head, forehead	q'uw-/q'ow-	forehead, brow
gub (?)	to stand, to erect	gub-/gob-	highest point, summit, top
gud$_4$-da, gud$_8$-da	short	k'ut'-/k'ot'-	to be small
gul	to destroy	q'ʷal-/q'ʷəl-	to strike, to hit, to cut, to hurt
gúm	to take hold of	k'um-/k'om-	to pack or press together > to grasp
gur	difficult, hard, severe, tough, arduous	k'ʷur-/k'ʷor-	to be harsh, severe, biting, cold
gur(-gur)	to rub off, to rub down, to grind	k'ʷur-/k'ʷor-	to crush, to grind
gur, gúr	to bend (tr.); to twist, to turn, to roll up	gur-/gor-	to turn, to twist, to wind, to wrap
gur, gur$_4$	hefty, thick	k'ʷurʸ-/k'ʷorʸ-	to be heavy, solid, bulky
gu-ru-un, kurin	blood	kʰur-/kʰor-	blood
gu$_4$, gud	ox, bullock	k'ʷuw-/k'ʷow-	bullock, ox, cow
gu-ul	to enlarge, to increase	q'ʷal-/q'ʷəl-	to swell, to expand
hal, hal(-hal)	to apportion, to allot, to distribute	ħal-/ħəl-	to divide, to allot, to apportion
ha-lam	to destroy, to demolish, to wreck	ħal-/ħəl-	to wear down, to weaken
har(-har)	to scratch, to scrape	ħar-/ħər-	to scratch, to scrape
hašhur	apple, apple-tree	ħasʸ-/ħəsʸ-	a tree and its fruit
ha-za	to seize, to grasp, to take hold of	ħac'-/ħəc'-	to seize, to grasp, to pick, to pluck
hul	to destroy	ħul-/ħol-	to destroy, to lay waste
hu-rí-in	eagle	ħur-/ħor-	falcon, hawk
inim	word	ʔin-im-/ʔen-im-	to say, to speak, to name; (n.) name
kàd, kad$_5$	to tie, to bind, to fasten	kʰad-/kʰəd-	to twist, to wind, to wrap, to bend
kal	to hold, to keep, to retain	kʰal-/kʰəl-	to guard, to watch, to hold (back)
kar	embankment, quay-wall	kʰar-/kʰər-	edge, side, bank
kur (?)	mountain	q'ʷur-/q'ʷor-	edge, point, tip, peak
kur$_5$	to cut, to separate, to divide	kʷʰur-/kʷʰor-	to cut
kur$_4$	to tie, to bind	kʷʰur-/kʷʰor-	to twist, to twine together, to bind
làh	to wash, to clean	laħ-/ləħ-	to make flow, to pour, to moisten

li	negative particle	ʔal-/ʔəl-	element of negation
luh, luh(-luh)	to wash, to clean	law-aħ-/ləw-aħ-	to wash, to clean
ma(-e)	I (Emesal)	mi-/me-	1st person singular pronoun stem
mah	to be or make great; to be much, many	maħ-/məħ-	to increase, to swell, to exceed
mar	to daub, to anoint	mar-/mər-	to smear, to annoint, to rub
me	abundant, plenty	maʔ-/məʔ-	to increase (in number), to be many
me-a, me-šè	where?, where to?	mi-/me-	interrogative pronoun stem
mur	to crush, to grind	mur-/mor-	to crush, to break, to destroy
mur	cry, scream, shouting, yelling, voice	mur-/mor-	to make a noise, to murmur
na	not	na/nə	negative/prohibitive particle
ne-en, ne(-e)	this	ni-/ne-	demonstrative stem
ner	prince	nir-/ner-	to be strong, manly, virile
nu, nu-	not, negative prefix	nu/no	negative/prohibitive particle
pála, pàla	clothes, clothing (of a god or a king)	pʰal-/pʰəl-	to cover, to hide, to conceal
pàr	to spread or stretch out	pʰar-/pʰər-	to spread, to scatter
pàr	to go or pass by, to go past	pʰar-/pʰər-	to go or pass, to go forth or out
peš	semen, sperm; descendant, offspring	pʰasʸ-/pʰəsʸ-	semen, sperm; male genitals; offspring
peš, peš$_5$, peš$_6$	to breathe, to respire, to blow	pʰasʸ-/pʰəsʸ-	to breathe out, to blow
peš$_5$	to break, to smash, to shatter	pʰasʸ-/pʰəsʸ-	to split, to cleave, to break, to shatter
pir	to wrinkle, to crumple	pʰir-/pʰer-	to twist, to turn
sahar	dust, sand, earth	cʰaħ-/cʰəħ-	to crush, to pound, to grind, to beat
sal	to set free, to release, to let go	cʰal-/cʰəl-	to empty, to leave, to leave behind
šár; šár-šár	to bring together; to organize	tʸʰar-/tʸʰər-	to advance toward an end or goal
šer	to tie, to bind	sʸir-/sʸer-	to twist, to turn, to bind
šum	to slaughter	tʸʰum-/tʸʰom-	to strike, to hit, to beat
šur	to pour out, to flow, to gush out	sʸur-/sʸor-	to surge, gush, flow, or spring forth
tab	to burn, to blaze; (n.) fever	t'ab-/t'əb-	to be or become warm; to heat up
tag	to touch	tʰak'-/tʰək'-	to touch, to push, to strike
tál	to be or make wide, broad; to spread	tʰalʸ-/tʰəlʸ-	to stretch, to spread, to extend
tar	to disperse, to scatter; to untie, to open	tʰar-/tʰər-	to spread, to spread out; to stretch
tar	to be distressed, troubled	tʰarʸ-/tʰərʸ-	to rub, to wear down
tum	abundance, plenty	tʰum-/tʰom-	to fill, to fulfil
ù-a, ù	woe!	way	exclamation
ubur	(wife's) breast	ʕub-/ʕob-	bosom, breast
ùl, ul$_4$, úlul	field, meadow	walʸ-/wəlʸ-	to be open; (n.) open space, field
unu, únu	dwelling, dwelling-place, residence	wan-/wən-	to stay, to remain
uru$_4$, ur$_{11}$(-ru)	to plow	wurʸ-/worʸ-	to scratch, to incise, to dig up
ur$_4$-ur$_4$	to burn up, to consume, to flame	wur-/wor-	to burn
u$_8$	ewe	ʕuw-/ʕow-	sheep and goats

za-e; zu	you; your	tʰi/tʰe	you
zag	to drive away, to expel	ʒag-/ʒəg-	to strike, to beat, to drive (away)
zar	to run, flow, leak, or spill out	ʒar-/ʒər-	to gush forth, to burst forth
zíl	to do good, to please; (adj.) good	c'il-/c'el-	to stretch out, to extend, to exceed

MOTHER TONGUE

JOURNAL OF THE ASSOCIATION FOR THE STUDY OF LANGUAGE IN PREHISTORY, ISSUE III (DECEMBER, 1997)

RECOMMENDATIONS FOR LONG RANGERS

Paul K. Benedict.

[Long-time ASLIP member and elder statesman Paul K. Benedict was taken from us by an automobile accident on July 21, 1997. The following recommendations were written during his last months, and we think it appropriate to print them here as the parting advice of a great long ranger. Editor] [1]

As an underlying principle, use only the earliest possible forms (EPFs). For this purpose, make a careful distinction between earliest and oldest: Archaic Chinese (AC) forms with final -o are some three thousand years older than cognate Proto-Tibeto-Burman (PTB) forms, but the latter are earlier. For example, Proto-Sino-Tibetan (PST) *ŋa `I' yielded ŋo in AC, but ŋa in Burmese, and so on.[2]

Three basic kinds of material are to be distinguished:

1. Isolates (e.g., Basque): Where there are significant dialectal differences, cite the forms but make sure to include an EPF, which can be either one of the cited forms or a reconstructed form. If the writer is unable to come up with an EPF, he should state so -- and say why not. Where signigicant dialectal differences are lacking, the root should be clearly indicated, through morphological analysis and/or internal reconstruction.

2. Related languages (when the proto-language is not yet reconstructed): Cite the forms, along with your suggested reconstructions and the analysis behind it. If it cannot be reconstructed, say so, and give the reason why not. At times, the reason could simply be the lack of comparative materials.

3. Reconstructured proto-languages (e.g., PST): Cite only the reconstructed forms, indicating the source. Make sure to cite updated

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[1] Editorial Note: The text has been very slightly edited by one editor. Another (HF) makes a few clarifications of important concepts at key points and lightly stresses the issues involved. All comments are based on many letters and conversations with PK.

[2] Editor's Note (HF): A key distinction for PK. `Earliest' means the most archaic in terms of development, the closest to the proto-form, or the proto-form itself. But `oldest' refers to chronology in dated years or the like. So in Semitic, Akkadian is easily the `oldest', yet Arabic is the `earliest' in preserving more of proto-Semitic. We politely suggest that PK may have mixed AC up with PST whose forms are both older and earlier by definition. We doubt that PK saw AC as older than PTB.

forms, e.g., PST myâk `eye' (Benedict 1976), not the *mik ~ *myak indicated at the later PTB level (Benedict 1972). This is close to the *mât indicated for Proto-Austro-Asiatic (PAA) by Pinnow's Munda analysis. (PAA lacks *my-, which could have yielded *m- with *-t < -k through assimilation). Again this is a properly "updated" form compared with Schmidt's earlier reconstructed *mat for Proto-Mon-Khmer (PMK), which he compared with Proto-Malayo-Polunesian *mata, without any explanation for the syllabic loss. Here again an updating is available: Proto-Austronesian *maCa, with C a cover symbol (Dyen), here the reflex of a lacking consonantal cluster, *pr, as shown by the related Proto-Tai *pra `eye', showing regular monosyllabizing (contrast PMK).[3]

As shown here, updating is of extreme importance, as are EPFs. Where more than one line of reconstruction is available (as notoriously in the case of Indo-European) indicate which line you are following and, in general terms, why. Do not wander from one line to another, looking for best possible fits.

Always carefully specify the <u>level</u> of a cited reconstructed form. A good portion of the roots cited in the index of Benedict (1972) are at the PTB level, not PST, e.g., *r-sa$_{\eta}$ `lizard' < PST *[r-]sa$_{\eta}$ (bracketed r- because it is not attested in AC).

In all three groups, derived forms should be cited as such, e.g., PTB *s-na `nose' (s- = a body part prefix), from PTB/PST *na `water source / headwaters / spring' via `snot source' (see Benedict 1972: 35, 177, etc.).

All manuscripts should be sent to Hal Fleming [Editor of <u>Mother Tongue</u> when this was written], with EPFs all in place, and unencumbered with hordes of non-EPFs to be weeded out. We owe this much to him, the spiritual *na of <u>Mother Tongue</u>.[4]

REFERENCES:

Benedict, Paul K. 1972. <u>Sino-Tibetan: A Conspectus.</u> Ed. by James A.Matisoff. Cambridge (UK): Cambridge University Press.
------- 1976. "Sino-Tibetan: Another Look". <u>Journal of the American Oriental Society</u> 96/2: 167-197.

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[3] Editor's Note (HF): PK loved acronyms and abbreviations more than anyone in our experience. He frequently abbreviated ordinary words or substituted known short forms for them. One of his favorites was `cf.' which he used as the verb `to compare'. This style meant that a letter from PK was often followed by hours of decoding time on the reader's side!

[4] Editor's Note (HF): Did he just reach up out of his grave and have the last laugh? He just called me a snot, right?

MOTHER TONGUE

JOURNAL OF THE ASSOCIATION FOR THE STUDY OF LANGUAGE IN PREHISTORY, ISSUE III (DECEMBER, 1997)

An Editorial for MOTHER TONGUE III.

Roger W. Wescott (January 31, 1998)

There are three major topics that I would like to address in this editorial:

I. Writings submitted to the Journal by contributors;
II. The editorial policy of the Journal;
III. My views on the reconstruction of prehistoric languages.

As a co-editor of Mother Tongue (MT), I look for three qualities in writings submitted by contributors. The first and most important of these is clarity. Muddled or ambiguous writing defeats the primary purpose of the contributor, which is to inform and persuade. It also thwarts the editors, who, in their effort to clarify obscure statements, cannot be sure that their rewriting of those statements expresses the intention of the authors. And it frustrates the readers, who, unable to extract meaning from a sub-mission, tend to skip over it and proceed to the next submission, in hopes that it will prove to be more readily comprehensible.

The second desideratum in writing is brevity. We live in a busy world which seems always to be getting busier. Long lead-ins to the core of the topic make readers impatient, while repetitions of information earlier provided becomes tedious.

The third mark of good writing is elegance. It is, however, a distant third. Writers who put felicity of expression before clarity and brevity too often produce texts marred by pretentious discourse. Elegance is the icing on the expository cake. When overapplied, it cloys.

* * * *

As regards editorial policy, I recommend that, as far as possible, we transcend exclusively in-group discussions of linguistic subclassification. Our articles should appeal not only to linguists with "long range" interests but also, and most obviously, to prehistorians, including those scholars whose interests overlap prehistory, such as historians, archeologists, and anthropologists. Beyond these, we should appeal to as many as we can of investigators in the humanities, social studies, and life sciences whose interests include the origin and development of that most distinctively human behavior pattern: language.

I further urge my colleagues to avoid vituperation, not only with regard to conventional "short range" linguists but also when expressing disagreement, however well-justified, with fellow long-rangers.

Finally, to clarify our overall vision of language in space and time, I suggest that we publish pictorial dendrograms, or "language trees," illustrating the relationship of macro-phyla, such as Nostratic and Amerind, to one another. Such illustrations could readily be adapted from work already done by Vitaly Shevoroshkin, Merritt Ruhlen, and others.

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No collective intellectual enterprise can be lauched and sustained without some consensus concerning basic assumptions and consequent methods. On the other hand, a consensus that amounts to unanimity is unlikely to generate fresh approaches to shared subject matter or new ideas for exploration. Each of our MT editors and contributors is likely to depart from the "long range" consensus on one or more issues.

One issue on which my views differ from those of most of my MT colleagues is that of the nature and antiquity of phonemes. I doubt that any proto-language prior to Common Indo-European had prosodic and vocalic as well as consonantal phonemes. I assume that the speech-forms of Proto-Nostratic consisted exclusively of consonants. I believe that the vowels selected to make these forms pronounceable were, in terms of their function, prosodic rather that phonemic. As a general phonic trait, vocalism provided syllabicity; but specific emotive colors were provided by phonetic differences between vowels. Contrasts of vocalic height or advancement connoted oppositions between diminution or augmentation, elevation or abasement, and the like. A monosyllabic proto-form like TK, meaning `limb extremity', for example, might designate `finger' when pronounced TIK but `hand' when pronounced TUK or `arm' when pronounced TÎK but `leg' when pronounced TAK.

I further opine that, even after vowels became phonemic, some segmentals may have been restricted in occurrence as regards what George Trager called phonic domains. In Proto-Indo-European, for example, the canonic e/o opposition may have belonged to what Trager termed microlanguage (speech in the most restricted sense), while the exceptional i/a opposition may have belonged to what I call allolanguage (deviant speech, embracing Trager's subdomains termed prelanguage, paralanguage, and metalanguage).

A second issue on which I differ from most of my colleagues is that of the extent of the phenomenon known as ablaut, or apophony -- that is, systematic alternation of phonemes. Most historical linguists recognize apophony only among vowels, as in such familiar sequences as the grammatical seriation sing/sang/sung or the (admittedly rarer) non-grammatical sequences among consonants, as in the non-grammatical seriations drip, dribble, drivel or call, gale, holler.

Granted that such apophony is obsolescent in contemporary languages, I find it commoner in earlier languages, especially (to judge by reconstructions) in prehistoric languages. My further finding, that, as we move back in time, consonant alternations become not only more frequent but more semantically significant, phonic oppositions signaling shifts in meaning. My guess is that originally all such alternation was meaningful and yielded antonyms but that, as phonemic meaning was increasingly transferred to morphemes, consonantal apophony increasingly produced synonyms rather than antonyms.

An example of meaningful apophony provided for Proto-Global by Alfredo Trombetti is kor, `eagle' (i.e., `high flying bird') as against gor, `crow' (i.e., `low flying bird'). An example of semantically emptied apophony is Proto-Global kwan/kwal `dog', as reconstructed by John Bengtson and Merritt Ruhlen.

In Proto-Nostratic as reconstructed by Vladislav Illich-Svitych and transmitted by Mark Kaiser, kap/kab, `seize', and rak/lak, `leg' are without semantic contrast. But, in Proto-Nostratic as reconstructed by Allan Bomhard, par, `surpass', bor, `protrude', and bol, `swell', do exhibit semantic contrast, the voiceless stop apparently connoting elevation and the vibrant lingual potential threat.

In Proto-Indo-European as reconstructed by Morris Swadesh and by me, there are dozens of examples of consonants that alternate in voice, tongue-position, and otherwise. Since only a minority of these examples exhibit semantic as well as phonic contrast, I will here confine myself to a handful of those which exhibit both types of contrast, as below:

pet	`wing'	ped	`foot'
kel	`warm'	gel	`cool'
lewk	`bright	lewg	`dark'
plew	`flow'	bhlew	`overflow'
wekw	`speak'	weghw	`vow'
wer	`water'	wel	`wet'
tewk	`thigh'	doyk	`toe'

Finally, I believe that the earliest form of affixation was infixation, which has increasingly been replaced by prefixation and suffixation in historic times. Infixes, I surmise, were initially iconic in nature, palatal glides and lateral linguals having connoted diminution while labio-velar glides and vibrant linguals connoted augmentation. By Proto-Indo-European times, most of this iconism had been lost, leading to synonymy between forms containing resonant infixes and forms lacking them, as below:

infix	prenuclear	postnuclear
y	g(y)ew, `chew'	de(y)k, `show'
w	s(w)eks, `six'	re(w)p, `snatch'

r	bh(r)eg, `break'	ka(r)p, `harvest'
l	k(l)ew, `hear'	ke(l)p, `grasp'
n	m(n)egh, `copious'	ghe(n)d, `get'
m		ge(m)bh, `jaw'

Such infixation, which had already ceased to be productive in early Indo-European times, is even less so in contemporary English. Nonetheless, the fact that it is not yet wholly obsolete is borne out, I think, by such patterns as those below:

infix	prenuclear	postnuclear
y	moo, mew	twit, tweet
w	hack, whack	gook/guk, guwk/
r	cackle, crackle	pup, purp
l	beep, bleep	yip, yelp
n		click, clink
m		clap, clamp

MOTHER TONGUE
JOURNAL OF THE ASSOCIATION FOR THE STUDY OF LANGUAGE IN PREHISTORY
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LONG-RANGER EXTRAORDINAIRE: SERGEI A. STAROSTIN

John D. Bengtson

I recently had the privilege of staying a few days with Sergei A. Starostin, a Council Fellow of ASLIP, and currently a Professor and Department Head of Historical Linguistics and Ancient Languages at the Russian State University of Humanities (Rossijskij Gosudarstvennyj Gumanitarnyj Universitet) in Moscow[1] and serves on the Editorial Board of the Moscow Linguistic Journal (Moskovskij Lingvističeskij žurnal). At the time of my sojourn (December 1995), Sergei was wrapping up a semester as a visiting lecturer at the University of Michigan at Ann Arbor. For part of that time we also had the pleasure of the company of another ASLIP Council (and Sergei's longtime comrade in the Moscow School), Vitaly Shevoroshkin.

At other times and places, Sergei has served as a teacher of Russian to foreign journalists, a field worker in Siberia (Ket) and the high Caucasus Mountains (Inkhokvari, Rutul, Tabasaran, etc.), a leading instigator in the sixties Moscow rock-and-roll movement, an early student of Aron B. Dolgopolsky (another Council Fellow), later and currently a leading figure (along with Shevoroshkin, Dolgopolsky, Vladimir Dybo, Anna Dybo, Sergei Nikolaev, Ilia Peiros, Eugene Helimsky, et al.) in the Moscow School of Comparative Linguistics, and now a world class leader in the deep reconstruction and classification of languages (= Swadesh's "Prehistoric Linguistics”). I am pleased to say I was regaled with an anthology of fascinating (and often hilarious) tales: for example, life with the Caucasic mountaineers (the ultimate `hillbillies'), intrigues among the colorful characters in the Moscow School, and being interviewed by the KGB about a smuggled Beatle record.

Probably all who have heard and read of Sergei's accomplishments may not know that he is known around the world as a leading authority in no less than four major linguistic areas: Japanese (and Korean), Chinese, Ket (in Siberia) and the (North) Caucasic languages. Sergei has never been content with just learning the language: in each case he also insisted on taking the language back as far as he can. Thus Japanese is taken back to Proto-Altaic (and deeper yet, to Proto-Nostratic[2] (1989a, 1991a), Ket to Proto-Yeniseian (1982, 1995a), Chinese to Old Chinese (1989b), further to Proto-Sino-Tibetan (Peiros and Starostin 1996), and the 35-odd Caucasic languages back to Proto-(North) Caucasic (Nikolaev & Starostin 1994).

Where proper reconstructions did not already exist, Sergei went to work and made them himself (P-YEN and P-Caucasic, the latter along with Nikolaev), or revised the ones that already existed in som form (P-Japanese, P-Altaic, P-Nostratic, Old Chinese, P-ST, the last along with Peiros). In a pivotal long-ranger document (1984, 1991b) he proposed a new macro-phylum to complement Nostratic in northerm and central Eurasia: Sino-

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[1] Moscow, Russia, not Moscow, Idaho, where Sergei and I learned (from the Internet) there is a Judge John Bengtson -- no relation that I know of.

[2] Editor's Note: Sources are listed only by date, the author being presumed to be Starostin. See References for full citation.

Caucasic (a.k.a. Dene-Caucasic), consisting of Caucasic, Yeniseian, and Sino-Tibetan.

But Sergei is no mush-minded Long-Ranger, no mindless lumper, no "fuzzy-thinking one-worlder", as any attentive reading of his work shows. It is only after painstaking research and reconstruction that Sergei comes to the conclusion that a classification is correct, based only on scientific evaluation of basic vocabulary, morphology, and phonology. Thus, he finds that Chinese, Ket, and Chechen (et al.) are ultimately classified together in the Sino-Caucasic macrophylum; on the other hand, Japanese and Korean are definitely not Sino-Caucasic, but rather Nostratic (with Indo-European, Uralic, Altaic, et al.); and Ainu, for that matter, belongs to neither. (In case there is any doubt, I totally agree with this threefold division.)

This is potent ammunition, I think, against the critics who allege things such as that Long-Rangers are bent on "hoovering up[3] all the isolates" into Sino(-Dene)-Caucasic, or that we are intent on "shoe-horning" Basque or Burushic into the same. If so, why would we not be joyous about finding a few "random resemblances" (as those between, e.g., Basque and Caucasic are sometimes alleged to be) between Basque and Ainu, and declare it to be Dene-Caucasic?

The fact is, as Sergei never tires of pointing out, that `random resemblances' have nothing to do with the science of paleo-linguistics. All resemblances have to be evaluated according to lexicostatistical as well as phonological methods. If the resemblances fall within the realm of cultural vocabulary (as, e.g., a huge proportion of Chinese cultural vocabulary in Japanese, or the similar case of Latin/Romance vocabulary in Basque), they are not relevant to genetic classification, but are explained as borrowing. If, on the other hand, the resemblances are greater within the 35-item list of highly stable basic vocabulary (devised by S.E.Yakhontov), than within the corresponding less stable 65-item list (provided the absolute number of the former is more than 5), genetic relationship is at least probable, and should be investigated further.

For Sergei, this further investigation concerns phonology: genetically related languages not only share resemblances among their basic vocabulary, but these resemblances, or at least most of them, should phonetically correspond in some regular manner. He is, like other good Long-Rangers, acutely aware that some of the most stable vocabulary exhibits puzzling irregularities (e.g., Indo-European `tongue', or `name'). For Sergei (and the rest of the Moscow School) phonetic regularity is the main factor that raises the status of a genetic hypothesis from merely "possible" to "highly probable" or even "proved."

These principles have come into play in several of Sergei's recent publications. In "Old Chinese Vocabulary: A Historical Perspective" (1995b) it is shown that Old Chinese and Caucasic share 13 "precise phonetic and semantic matches" in Yakhontov's 35-word list, and Old Chinese and Yeniseian share 9 items. For the rival Chinese-Austronesian hypothesis

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[3] Editor's Note: Hoover is/was a kind of vacuum cleaner, here used as a verb roughly meaning `to suck it all up'. Shoehorning refers to the shoe horn for getting big feet into small shoes.

(recently associated with L. Sagart, and others) Sergei finds only four matches, and six matches in a Chinese-Indo-European (e.g. E. Pulleyblank) comparison. The inevitable conclusion is that the results for Caycasic and Yeniseian strongly indicate genetic connection (for example, Russian and English share 19 items on this list), while the results for Austronesian and Indo-European are very low, and close to the threshold of chance resemblance. (The minimal number is 5 shared items.) The relationship in the latter two cases "may exist (it is well known that no genetic hypothesis can be ultimately disproved), but on an extremely deep chronological order." (See, e.g., Starostin 1989a).

In the case of Basque-Caucasic comparisons, Sergei (1996) finds that after erroneous and dubious comparisons have been eliminated, there still remain about 74 (of about 272 discussed by R.L.Trask [1995] that are "quite plausible ... satisfactory and interesting," or "the most significant group: a small number [19] of comparisons ... belonging to Swadesh's 100-word list." Of these latter 19 comparisons, Sergei further finds that 13 belong to Yakhontov's 35-word list. Sergei finds this to be "a significant result", for the probable genetic connection of the languages, both because the 13 items rank well within the range of 5-15 required for distant relationship (1995b: 226), and because the higher percentage within the 35-word list (37%) than in the 65-word list (9%) is statistically typical of genetically related languages.

It is typical of Sergei's cautious, scientific approach that he does not consider this "significant result" enough to fully prove the genetic affiliation of Basque and Caucasic. The most he can say as yet is that he is "inclined to agree" that Basque and Burushic are related to Caucasic, but the "uncertainty of comparative phonology is the only factor that still keeps me from enthusiastically including Basque in Sino-Caucasian (or Dene-Caucasian)". (How to remedy this lack is one of the many topics Sergei and I discussed during my visit.)

Similar statistical methods are used in Sergei's (1991a) book "The Altaic Problem and the Origin of the Japanese Language" (in Russian). In my opinion, this book is the deepest and strongest collection of evidence for the reality of the Altaic family of languages (Turkic, Mongolic, Tungusic, Korean-Japanese; but not Ainu).

When we think of the living Long-Rangers who have made lasting contributions to genetic classification over enormous geographic spaces, two stand out: Joseph Greenberg in the West, and Sergei Starostin in the East. Quite naturally, there are significant differences in each scholar's interpretation of linguistic methods, and each has an individual approach to the problems of paleo-linguistics. But ultimately their results, the deep classification of the languages of Eurasia, agree to a remarkable degree.[4] The amazing thing is that the man who was born the year Stalin died[5] is little over half the age of the man born during the presidency of

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[4] A salient exception is Ainu (see above).

[5] Sergei's son, George Sergeyevich Starostin, was born on July 4, 1976. He is now the youngest member of the Moscow School, with an article on the Yeniseian verb in the Moscow Linguistic Journal (see References).

Woodrow Wilson. I hope and pray Sergei Starostin will have at least as many more years to practice his highly scientific form of linguistics.

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Starostin, G.S. 1995. "A reconstruction of the Proto-Yeniseian verbal system." (In Russian) The Moscow Linguistic Journal 1: 129-173.

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----- 1984. "Gipoteza o genetičeskix svjazjax sinotibetskix jazykov s enisejskimi i severnokavkazskimi jazykami." In Lingvističeskaja rekonstrukcija i drevnejšaja istorija vostoka 4: 19-38. Moscow [See Starostin 1991b]

----- 1989a. "Nostratic and Sino-Caucasin." In Explorations in Language Macrofamilies, ed., by V. Shevoroshkin, 42-66. Bochum: Brockmeyer.

----- 1989b. Rekonstrukcija drevnekitajskoj fonologičeskoj sistemy. [Reconstruction of the Old Chinese phonological system] Moscow: Nauka.

----- 1991a. Altajskaja problema i proisxo denie japonskogo jazyka. [The Altaic problem and the origin of the Japanese language.] Moscow: Nauka

----- 1991b. "On the hypothesis of a genetic connection between the Sino-Tibetan languages and the Yeniseian and North Caucasian languages." In V. Shevoroshkin, ed., 12-41. Bochum: Brockmeyer. [Translation of Starostin 1984, by W.H. Baxter III.]

----- 1995a. Sravnitel'nyi slovar' enisejskix jazykov. [Comparative dictionary of the Yeniseian languages.] Ketskij sbornik [Studia Ketica], ed., by S.A.Starostin, 17-313. Moscow: škola Jazyki Russkoj Kul'tury.

----- 1995b. "Old Chinese Vocabulary: a historical perspective." In The Ancestry of the Chinese Language (Journal of Chinese Linguistics, Monograph Series No.8, ed., by William S.-Y. Wang, 225-251. Berkeley: Journal of Chinese Linguistics.

----- 1996. "Comments on the Basque-Dene-Caucasian Comparisons." Mother Tongue (Journal) II: 101-109.

Starostin, S.A. and Merritt Ruhlen. 1994. "Proto-Yeniseian Reconstructions, with Extra-Yeniseian Comparisons." In On the Origin of Languages, by Merritt Rushlen, 70-92. Stanford: Stanford University Press. [Translation of parts of Starostin 1982, with comparisons added by the second author.]

Trask, R.L. 1995. "Basque and Dene-Caucasian: a critique from the Basque side." Mother Tongue (Journal) I: 3-82.

MOTHER TONGUE

Journal of the Association for the Study of Language in Prehistory
Issue III (December 1997)

Some Recent Publications Of Interest To Long-Rangers

John D. Bengtson
Minneapolis, MN

Blazhek, Václav, and John D. Bengtson. 1995. "Lexica Dene-Caucasica." Central Asiatic Journal 39 (1): 11-50. (Addenda and corrigenda in same volume: 39 (2): 161-164)

This is the first extensive collection of Dene-Caucasic etymologies since the 1991 book Dene-Sino-Caucasian Languages (ed. By V. Shevoroshkin, Bochum, Brockmeyer). It includes a provisional table to phonological correspondences among Basque, Caucasic, Burushic, Sino-Tibetan, Yeniseian, and Na-Dene. The 219 lexical comparisons involve those six languages, and, tentatively, material from Sumerian and Kusunda.

Chirikba, Viacheslav A. 1996. A Dictionary of Common Abkhaz. Leiden: (no publisher cited).

Chirikba is a native Abkhazian who has done extensive field work on his native language, and on other West Caucasic languages.(See his book on West Caucasic, reviewed in this volume.) Chirikba is also known for his work comparing Basque and Caucasic.

Dybo, Anna V. 1996. Semantičeskaja rekonstrukcija v altajskoj etimologii: somatičeskie terminy (plečevoj pojas). Moscow: škola "Jazyki Russkoj Kulytury."

Anna, daughter of Vladimir A. Dybo, is well known as an expert in Altaic languages. This book [in Russian] concentrates on semantic reconstruction of words denoting body parts.

Fähnrich, Heinz, and Surab Sardshweladse. 1995. Etymologisches Wörterbuch der KartwelSprachen. (Handbüch der Orientalistik 24.) Leiden/New York/Köln: E.J.Brill.

This etymological dictionary of Kartvelian (formerly "South Caucasic") languages supplements the earlier (1964) dictionary by G.V. Klimov (in Russian).

Hegedüs, Irén, Peter A. Michalove, and Alexis Manaster Ramer (eds.). 1997. Indo-European, Nostratic, and Beyond: Festschrift for Vitalij v. Shevoroshkin. Journal of Indo-European Studies, Monograph Number 22.) Washington, DC: Institute for the Study of Man.

This Festschrift honoring leading Long Ranger and ASLIP Council Fellow Vitaly (Vitalij) Shevoroshkin contains a bibliography of Vitaly's work, and 23 articles by other Council Fellows (Raimo Anttila, Joseph H.Greenberg, Karl Heinrich Menges, Sergei A. Starostin), prominent Long Rangers (Václav Blazhek, Irén Hegedüs, Vyacheslav Ivanov, Mark Kaiser, Alexis Manaster Ramer, Peter A. Michalove, Vladimir Orel, Ilya (Ilia) Peiros, Merritt Ruhlen, Alexander Vovin), and other scholars not thought of as Long Rangers (J.C.Catford, Eric P. Hamp, et al.). This book will be reviewed soon in Mother Tongue.

Nikolaev, Sergei L., and Sergei A. Starostin. 1994. A North Caucasian

Etymological Dictionary. Moscow: Asterisk Press. 1406 pages.

The first comparative dictionary to treat the (North) Caucasic family as a whole. It includes an extensive (160 pp.) comparative phonology of the family and its subgroups, and more than 2000 etymologies. Starostin (on ASLIP's Council of Fellows) and his colleague, Nikolaev, have been working on this project since the late seventies. A interesting account of its eventual publication is given by Starostin in the foreword.

Peiros, Ilia. 1995. Katuic Comparative Dictionary. (Pacific Linguistics Series C, no.132) Canberra: Research School of Pacific and Asian Studies, Australian National University.

Peiros, formerly at Moscow and now at the University of Melbourne, has produced the first comparative dictionary of this family, a subgroup of the vast Austroasiatic phylum.[1]

Peiros, Ilia, and Sergei A. Starostin. 1996. A Comparative Vocabulary of Five Sino-Tibetan Languages. (Six fascicles.) Melbourne:University of Melbourne, Department of Linguistics and Applied Linguistics.

This is the first etymological dictionary of the vast Sino-Tibetan phylum of languages. Its 2,637 lexical comparisons systematically include Chinese, Tibetan, Burmese, Jingpo, and Lushai; other Sino-Tibetan languages are cited in some etymologies. (See my review, forthcoming in the Journal of Chinese Linguistics.)

Ruhlen, Merritt. 1997a. L'origine des langues: Sur les traces de la langue mère. [The Origin of Languages: On the Trail of the Mother Tongue.] Translated by Pierre Bancel. Paris: Editions Belin.

This is essentially a translation of Ruhlen's The Origin of Language. Tracing the Evolution of the Mother Tongue (1994, New York: John Wiley & Sons), with the addition of Bengtson & Ruhlen's "Global Etymologies" (published in Ruhlen's other 1994 book: On the Origin of Languages: Studies in Linguistic Taxonomy, Stanford University Press), and a preface by André Langaney, director of the Laboratory of Biological Anthropology of the Museum of Man [Directeur du laboratoire d'anthropologie biologique du musée de l'Homme]. Merritt has found himself amid a flurry of Long Ranger activity in France: see also the next entry.

Ruhlen, Merritt. 1997b. "Une nouvelle familie de languages: le déné-caucasien." Pour la science (Edition française de Scientific American), Octobre 1997: 68-73 (Dossier hors-série).

In what is probably the first article in French about the Dene-Caucasic macro-family, Ruhlen discusses the history of the concept (giving the most credit, as a pioneer, to Edward Sapir), a resumé of recent developments by other scholars, and provides several etymological examples supporting Dene-Caucasic in general, and the proposed subgroups Macro-Caucasic (Basque + Caucasic + Burushic), and Ruhlen's recent "Northern

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[1] Editor's Note: According to Ruhlen's GUIDE, Katuic is a group of 28 languages, a sub-branch of the `East' branch of Mon-Khmer, one of two moieties of Austroasiatic. Few, if any, Katuic languages are well-known.

Dene-Caucasic" (Yeniseian + Na-Dene).

This article is part of a special issue: "Les langues du monde," which also includes contributions by ASLIP Council Fellows Joseph H. Greenberg, Colin Renfrew, and L.Luca Cavalli-Sforza; as well as by other eminent scholars, such as Peter Bellwood, Tamaz Gamkrelidze, Vyacheslav Ivanov, William S.-Y. Wang, et al.

Shevoroshkin, Vitaly (Festschrift): see Hegedüs, et al.

Starostin, Sergei A. 1995. "Sravmotelymyj slovar' enisejskix jazykov." in Ketskij Sbornik (Studia Ketica), ed. by S.A.Starostin, pp. 176-315. Moscow: škola "Jazyki Russkoj Kulytury."

The first etymological dictionary of the Yeniseian language family of Siberia (Ket + the extinct Yug, Kott, Assan, Arin, Pumpokol, etc.). It includes external comparisons with Caucasic and Sino-Tibetan, consistent with the author's Sino-Caucasic (= Dene-Caucasic) hypothesis.

Wang, William S-Y. (ed.) 1995. The Ancestry of the Chinese Language. ((Journal of Chinese Linguistics, Monograph Series Number 8.) Berkeley: Journal of Chinese Linguistics.

This collection of articles features a debate between competing hypotheses of the origin of the Chinese language, ranging from Chinese + Indo-European (E.G. Pulleyblank), to Chinese + Austronesian (Laurent Sagart, Zhengzhang Shangfang), to the most commonly accepted model, Chinese + Tibeto-Burman (= Sino-Tibetan: William H. Baxter, Paul Jenkuei Li, Sergei Starostin, et al.). Starostin ("Old Chinese vocabulary: A historical perspective") goes even farther in connecting Chinese with (North) Caucasic and Yeniseian (= Dene-Caucasic).

* *

A new journal: Moscow Linguistic Journal / Moskovskij Lingvisticheskij zhurnal, is published by the Russian State University of the Humanities (Rossijskij gosudarstvennyj gumanitarnyj universitet), under the direction of A.N. Vardulin, M.N. Krontauz, E.V. Muravenko, T.A. Muravyeva, N.V. Pertsov, and S.A. Starostin (see above). The editors may be contacted at: 125267, Moskva, Miusskaja pl. 6, korp. 2, FTiPL, Redakcija MLZ. (E-Mail) < apld@rggu.msk.su > The first issue (Volume 1, 1995) was edited by Sergei A. Starostin, and revolves around the "Moscow School of Comparative Linguistics," the Long Ranger group associated with V.M. Illich-Svitych, A.B. Dolgopolsky, V.A. Dybo, and others. Here I reproduce the English version of the table of contents of Volume I:

pages
7-9 "The Moscow Linguistic Journal": concepts and prospects. Editorial.
10-13 The Moscow school of comparative linguistics.
S.A. Starostin (Moscow)
14-33 The Nostratic vowels in Indo-European. A.B. Dolgopolsky
34-40 The Proto-Uralic origin of consonant gradation.
Evgeny Helimski (Moscow - Budapest)
41-50 On Indo-European triune velars and Nostratic front vowels.
Alexis Manaster Ramer (Wayne State University)
51-98 On Illich Svitych's Nostratic Theory. A Review Article on: [Illich-

Svitych 1971, 1976, 1984]. Alexis Manaster Ramer (WSU).
99-116 Hamito-Semitic, Sinocaucasian, Nostratic.
V. Orel (Jerusalem)
117-128 Hamito-Semitic and Nostratic: additions to the Nostratic etymologies and some new comparisons. V.Orel (Jerusalem)
129-173 A reconstruction of the Proto-Yeniseian verbal system.
S.A.Starostin [a misprint for George S. Starostin, Sergei's son!]
174-190 The historical position of Bai [2]
S.A. Starostin (Moscow)
191-235 On vowel length and prosody in Altaic languages.
S.A. Starostin (Moscow)
236-279 Accentuation processes in the languages of Teda-Kanuri group and the problem of origin of paradigmatic accent systems.
V.A. Dybo (Moscow) [3]
280-289 Once more about the co-ordination of the Nostratic theory with the results of Turkic studies. A.V. Dybo (Moscow)
(A. = Anna)

[Note that only five of the articles are in English (pp.34-50, 174-235, 280-289), the rest being in Russian. Some of the articles are accompanied by very brief abstracts (one to three sentences) in English and Russian.]

* *

Books Wanting Reviewers:

We have recently received a number of books from publishers whose gifts are meant to produce reviews, aka publicity, naturally. All are from Oxord University Press. Any member wanting to review one of these books in the Newsletter or next year's Journal should write to the Secretary of ASLIP, 16 Butman Avenue, Gloucester, MA 01930-1006 USA. All requests are responded to on a `first come, first served' basis. Here is the short list.

Lyle Campbell. 1997. American Indian Languages: The Historical Linguistics of Native America. New York/Oxford, Oxford University Press. $75. It is reviewed in this issue by Ken Hale of M.I.T, a `mainstream' Americanist. The book dismisses Greenberg as expected. .

Gerry T.M. Altmann. 1997. The Ascent of Babel: An exploration of language, mind, and understanding. (Despite the misleading major title, the book is concerned primarily with the minor title. Not so much prehistory as psycholinguistics.) Pp. 257. Oxford University Press.

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[2] Editor's Note: A rare event, a paper on Bai, the orphaned twin of Chinese, an important language for taxonomy and prehistory. Triangular controversy among (a) Sagart, (b) Chinese linguists, and (c) Starostin and before him, Benedict. As one would suspect the key issue is Sino-Bai similarities -- borrowings or cognates? In the Wang citation (above) an article by Starostin on Bai appears.

[3] Editor's Note: In the earliest issues of Mother Tongue, née Circulars 1-3 of the Long Range Comparison Club, considerable credit was given to Professor Dybo for his role along with Aharon Dolgopolsky and Illich-Svitych in starting Nostratic studies in Moscow, leading thence to the Moscow School of Comparative Linguistics.

Bernd Heine. 1997. Cognitive Foundations of Grammar. (The title labels the book accurately.) Pp.185. Paperback. Oxford University Press.

Alessandra Giorgi and Fabio Pianesi. Tense and Aspect: From Semantics to Morphosyntax. Oxford Studies in Comparative Syntax. Pp.319.

MOTHER TONGUE
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HARDWARE SYMPOSIUM: DISCUSSING THE ORIGIN OF LANGUAGE

This could also be called a many-sided reciprocal book review conglomerate. (But who would want to use such language?)

Five talented researchers into the emergent phenomenon called human natural language, or simply human language, have presented some of their views on the basic question of how it all got started. The vehicle for the discussion is reciprocal review of books written by each other on the subject. Since the demands of time and other projects has greatly reduced the original ambition of this symposium, we settled for an expression of views in general terms, rather than more detailed reviews of each of the several books written by these scholars. Yet we still learn a lot and at least one major theory, or theoretical approach, would appear to have been reduced in credibility by a plurality of opposing views. As we go on, you will see what this means.

We begin with Stephen Zegura (University of Arizona), an anthropologist with credentials in biological anthropology, including biogenetics. Steve is known to long rangers for his participation with Greenberg and Turner in the tripartite classification of native Americans, and for his recent article with Mike Hammer on the Great Diaspora, reported in the Newsletter (MT-30). Steve's focus in this symposium is on Philip Lieberman's book, EVE SPOKE.

Next comes Philip Lieberman himself (Brown University), linguist - indeed Chomsky's first student to receive the PhD - but also a serious student of neuroanatomy. Phil has been one of the founders and pioneers in hardware studies, along with Eric de Grolier and the late Jan Winter. His contribution to the symposium is a brief rebuttal to Zegura as well as a more detailed critique of Stephen Pinker, nowadays the leading advocate of Chomskyite, also called Neo-Chomskian, theory. He finishes with a comment on the recent discussion of the *planum temporale* in MT-30.

The third discussant, Merlin Donald (Queen's University, Kingston), is a psychologist who focuses on psycholinguistics. His book, ORIGINS OF THE MODERN MIND: Three Stages in the Evolution of Culture and Cognition(1991), is one of the primary books which the symposium concentrates on. His remarks, touching on various books and theories, are brief but remarkably incisive.

The fourth scholar, Tecumseh Fitch (Harvard University), student of Phil Lieberman, masterfully integrates much of the discussion.

Finally, Terrence Deacon (Boston University), provides an eagle's eye view of the debates. He is basically a biological anthropologist cum neuroanatomist. His book, THE SYMBOLIC SPECIES: The Co-Evolution of Language and Brain (1997), draws heavily on cultural concepts.

MOTHER TONGUE
JOURNAL OF THE ASSOCIATION FOR THE STUDY OF LANGUAGE IN PREHISTORY
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Review of Philip Lieberman's Eve Spoke

by Stephen L. Zegura

As I read this short tome intended for a popular audience, I kept getting flashbacks to the skit in Monty Python's hilarious movie, The Holy Grail, wherein John Cleese leads the formidable group known as "The Knights That Say N[i]". Since so many of the Flying Circus protagonists had Oxbridge educations, perhaps this famous knightly shibboleth possessed a sophisticated tongue-in-cheek quality that I completely missed during my approximately dozen viewings of this movie. . . a speculation cautiously reinforced by the choice of the exceptionally tall Cleese as the leader of this cadre strangely obsessed with artistic shrubbery. Could this really be a fanciful linguistic, biological, and cultural allegory for the "superiority" of Homo sapiens over their Neandertal cousins? Indeed, better speaking ability, increased stature and a blossoming of various art forms have all been repeatedly associated with the appearance of modern Homo sapiens in Europe by numerous authorities. My particular interpretive use of this skit was, of course, unintended by its creators and herein lies one of the crucial messages of Lieberman's book: the reason for origin of a trait may be very different than its current utility. Specifically, human speech and language may be exaptations, whereby brain structures originally adapted for tasks such as tool making that require especially precise motor control were coopted to produce our functional language system. This evolutionary process unfolded gradually so that not just one, but rather many "Eves" spoke. Thus, rudimentary speech and language abilities can be projected into the past probably at least as far back as Homo erectus and perhaps even farther with the caveat that these earlier "Eves" spoke less and less like we do.

A "lay" audience will learn a great deal of value by reading five of the six chapters in this book. The fourth chapter focusing on paleoanthropology is more problematic and will be discussed later. Topical and thematic strengths of the book include: the brief sections on chimpanzee "culture" involving tool-kits, pedagogy, hunting, warfare, politics, cognitive abilities, and language acquisition experiments; the presentation and comparison of various reconstructed primate supralaryngeal vocal tracts; the concise rendering of some glaring limitations of theories associated with the existence of a language organ and/or gene and a morality organ and/or gene (and the dangers of genetic determinism in general); the consistent theme throughout the book that underlying genetic factors only represent potentialities and that different environments can result in the production of different observable outcomes based on similar genetic substrates (Note: This is the classic "reaction norm" concept in genetics though it is never explicitly so stated in the book.); the Darwinian focus on preadaptation and natural selection in the language arena; the debunking of some of the more preposterous claims of the Chomskian school and the trashing of Terrace's trashy statements about Project Washoe; the stress placed on general associative processes for language learning; the extension of the classical model of Broca's and Wernicke's areas as the language associated brain regions to include subcortical basal ganglia, the cingulate gyrus, the prefrontal cortex, and other inter-connected neural circuits as a more realistic portrayal of the many diverse components of our functional language complex; the correction of an earlier erroneous conclusion once championed by the author equating the adult Neandertal vocal tract with that of a newborn human; the clear temporal exposition of the complex ontogenetic processes associated with the descent of the larynx in humans; the admission that just because Neandertal speech may have been nasalized and may have lacked the [i], [u], [a], [k], and [g] sounds does not imply that Neandertals could not speak or did not have language; the explication of the non-conscious operation by which we use the vowel [i] to estimate vocal tract lengths so that complex formant frequencies can be properly decoded when we listen to different speakers; and the admonition against uncritically accepting the commonly held theory that adult humans are neotenous. An unrelated but not unimportant final consideration is the obvious humanity of the author. . . a quality that shines through the pages of the text and makes the book an especially satisfying read.

Now I turn to Chapter 4 which has many of the same deficiencies and oversimplifications as my own lecture notes of just a few semesters ago devoted to the hominid fossil record. The rapid pace of new finds, new dates, and new interpretations has made our family tree into a very thorny bush, indeed. Unfortunately, this accelerating complexity has been particularly characteristic of the time period between the actual writing of Eve Spoke and its appearance in book form. Not only did 1997 witness the replicated extraction and sequencing of Neandertal mitochondrial DNA but papers detailing the oldest stone tools and the momentous Atapuerca remains from Spain appeared. Rather than going through Chapter 4 page by page and quibbling about every little nit, the general tenor of my reservations should become clear as I discuss some of the new findings and their influence on the interpretive matrix of hominid evolution. A number of highly competent review articles concerning the hominid fossil record aimed at a non-specialist audience graced the pages of National Geographic, Scientific American, Discover, and Natural History during 1997 while more scholarly papers covering the same material were published in journals such as Science, Nature, the Journal of Human Evolution, and the American Journal of Physical Anthropology. Most (but certainly not all) of these dozens of papers came to the same general conclusion: The "splitter" interpretation of our evolutionary past, wherein there were numerous, often co-existing, hominid species over the last five million years is preferable to the "lumper" view of a much less speciose record. The crux of the debate involves the differential apportionment of morphological diversity to the inter- vs infra-specific categories of variation with the disagreements often conditioned by the very different species concepts adopted by the protagonists.

Some examples of this "splitting" trend within the genus Homo usually but not invariably associated with the adoption of the phylogenetic species concept and with Hennigian methods in general include: the separation of the larger-brained Homo habilis specimens into a different taxon, Homo rudolfensis; the separation of some of the early smaller-brained African Homo erectus specimens into the taxon, Homo ergaster; the increased acceptance of the taxon, Homo ergaster, with extended spatial and temporal ranges; the possible geographic restriction of Homo erectus to Asia with the African and European Homo erectus specimens transferred to Homo ergaster and Homo heidelbergenesis, respectively; the extension of the temporal span of Homo erectus in Southeast Asia to as recently as 27,000 to 53,000 years ago, thereby implying the co-existence of these Javanese specimens from Ngandong and Sambungmacan with modern humans who had already reached Australia by ca. 50,000 - 60,000 BP; the naming of the Gran Dolina Atapuerca fossils as a new species, Homo antecessor, tentatively linked phylogenetically to the earlier Homo ergaster; the recognition of Homo heidelbergensis as a possible common ancestor of both Neandertals and modern humans or as the direct descendant of Homo antecessor and the direct ancestor of only Homo neanderthalensis; and the return of the Neandertals to their own species, Homo neanderthalensis, based on genetic as well as morphological grounds. As the number of taxa in the genus Homo proliferate, one wonders if less than perfect speech and language skills may have served as isolating mechanisms throughout the history of our genus.

Given the importance ascribed to stone-tool making for the eventual appearance of speech and language in Lieberman's book (as well as in many of the pioneering works by the neuro-psychologist, Doreen Kimura) the thousands of recently published well-dated Oldowan-like tools from the Gona River region in Ethiopia have pushed the earliest known stone-tool manufacturing back to between 2.5 and 2.6 million years ago. Two principal candidates have been put forward as the responsible hominid group: the robust australopithecines and the genus Homo. Both taxa have their champions in the recent literature and it is certainly possible that some members of both taxa could have made simple stone tools given the functional morphology of their respective hands, although at present the weight of the evidence argues in favor of our "guys" (the genus Homo).

What is becoming abundantly clear is that there were many hominid out-of-Africa dispersal events extending back almost 2 million years to the end of the Pliocene. The entire Pleistocene record is best viewed as a palimpsest with the oft-postulated migration of modern Homo sapiens from Africa through the Levant around 100,000 years ago as just one of many such population movements, albeit an especially significant one for the hominids subsequently encountered during their trek. In terms of historical reconstruction in linguistics the sad fact is that a time depth of 100,000 years is well beyond current methodological capabilities for discovering exactly what this "Eve" had to say to her friends and family, either at home in Africa or on the road in Eurasia.

There are a few nits buried in the book that I find myself constitutionally incapable of passing over silently. To wit, human cytogenetic convention has changed recently such that syndromes named after a person who did not have the condition, no longer use the apostrophe (i.e., Down's Syndrome is now properly designated as Down Syndrome). Pan paniscus and Pan troglodytes are obviously not to be considered "geographically isolated sub-species" as claimed given that two distinct species names are employed and the fact that the vast majority of the appropriate specialists consider them to be separate species. Numerous times throughout the text humans are inadvertently tossed out of the order Primates because of distinctions made between humans and primates rather than between humans and non-human primates. Although evolutionary forces (i.e., natural selection) can operate on the phenotype of the individual, only populations can evolve in the Neo-Darwinian biological sense of the term. The statement: "Biological evolution operates on individuals." is an oversimplification that could cause real confusion for the intended audience of this book. My vote for the most curious statement in the book is the following: "The fossil record in 1973 also hadn't yet revealed early hominids like Homo habilis who clearly had invented stone tool technology. . ." given that this taxon was officially named and described in 1964 and that the one-time "cover-boy" for this taxon, ER 1470, was discovered in 1972! Somehow the fact that both Oldowan and Acheulean tools belong to the Lower Paleolithic seems to have been unappreciated given statements such as: "Lower Paleolithic tools clearly are more refined than Oldowan tools." The graphics in Chapter 4 associated with Figures 4-4 and 4-5 do not match the text descriptions. Figure 4-4 is missing the lateral view of a chimpanzee skull. Figure 4-5 purports to show: "The base of the skull of a chimpanzee with the skeletal measurements that Laitman made directly from the skulls." Instead, the actual Figure 4-5 is a drawing of a modern human tongue placed in a Neandertal mouth! There are a few typos in the text mostly having to do with reference dates but one typo sticks out and requires mentioning: "Harry Jerrison at UCLA, one of the world's leading authorities on brain evolution. . ." spells his name "Jerison". On a lighter note, I fail to see how a 145 pound male Neandertal standing less than five feet seven inches tall would make a superb football linebacker. Perhaps it might work on Friday night at the high school level but certainly not on Saturday (college) or Sunday (the NFL)[1]. A better position for our erstwhile athlete would be as part of the "wedge" on special teams!

In sum, Eve Spoke is a fun read but don't believe everything she is purported to have revealed, said, or not said. Rather than the biblical aphorism: "in the beginning was the word" which appears more than once in this book, it is just as likely that some emotionally laden vocal equivalent of the "raspberry" or gestural equivalent of the single-digit salute (so popular on our crowded streets and highways today) were also part of the early hominid communicative repertoire!

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[1] Editor's Note: As suggested earlier in the Newsletter, and drawing on David Pilbeam's discussion of musculature, he could still be a quarterback.

Lieberman responds to the Critique of EVE SPOKE. Then overleaf he makes his own critique of Steven Pinker's book. That is followed by his commentary on the planum temporale.

THE FOSSIL RECORD, TOOLS, AND LANGUAGE

Philip Lieberman (Brown University)

The message that I wished to convey in EVE SPOKE; HUMAN LANGUAGE AND HUMAN EVOLUTION regarding the fossil record, tools, and language was that it is not possible to make any really meaningful inferences concerning language from the stone tools that are associated with hominid fossils. For example, while Oldowan tools occur 2 million years ago while Australopithecine hominids lived, they were still being made 90 years ago by modern human beings who had normal brains, normal cognitive ability and fully developed languages. If we were to attempt to correlate technology with cognitive ability we would have to conclude that everyone's now much smarter than Benjamin Franklin. The fact is that technology derives from culture as much as inherent cognitive ability.

Nor can we say much about a person's cognitive ability by looking at his brain. Absolute brain size probably is a very rough index of intelligence, but behavior is the only measure of intelligence that is presently available. Therefore, the short account of hominid evolution in EVE SPOKE did not attempt to relate present controversies involving putative species. □

MOTHER TONGUE

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The Chomskian Position: A critique offered by Philip Lieberman
of
The Language Instinct: How the Mind Creates Language. STEVEN PINKER.

William Morrow, New York, 1994. 494 pp. illus. $ 23.00.

In the early decades of the present century the Swiss linguist Saussure started the study of language down a slippery path when he declared that the true objective of linguistic research was to understand certain phenomena that reflected "la langue." Other linguistic data, "parole" could safely be ignored. This principle governing the relationship between data and linguistic theory was codified and intensified in the Chomskian era. Certain selected data reflect linguistic "competence," other data "performance" effects that can be ignored. In practice, linguists often base their theories on a limited range of data that best support the current version of the theory, ignoring counter-evidence. Therefore, is not surprising to find a lack of balance in the arguments and data presented by Pinker. His goal is to convince readers of the truth of Noam Chomsky's claim that a detailed set of "building blocks," the Universal Grammar (UG), specifying the syntax of all human languages is built into our brains.

Pinker provides useful, understandable accounts of recent versions of Chomsky's generative grammar. In attempting to cover a broad spectrum of relevant biological data and theory Pinker occasionally is in error, as when he discusses the physiology of speech production (the vocal tract does NOT "amplify" any frequencies nor can the resonances of the vocal tract be assigned to specific portions of the mouth and pharynx. However, the major deficiency of THE LANGUAGE INSTINCT is that Pinker selectively picks and chooses data that support the Chomskian claim. The reader unfamiliar with the field has no way of knowing that other data often refute the specific claims presented by Pinker, or indeed when data are misrepresented. This unfortunately mars otherwise useful accounts that Pinker presents of studies on the reconstruction of "dead" languages by Joseph Greenberg and his colleagues, the deficits and neural bases of aphasia, the evolution of human beings and human language, and many other current issues.

For example, the Chomskian position is that innate neural language "modules" exist only in human beings. The language modules, furthermore, are "encapsulated" entities independent of other modules that regulate other aspects of behavior. Therefore, Pinker (p. 263) discusses experimental data that show that human infants categorically perceive the acoustic features that differentiate the "voiced" consonants [b], [d], and [g} from their "unvoiced" counterparts [p], [t] and [k]. What Pinker does not note is that chinchillas also perceive these sound contrasts in like manner. The innate mechanism appears to be a property of the mammalian auditory system.

Pinker's discussion of a deaf child acquiring American Sign Language (ASL) from parents who are imperfect models (pp. 38-39) is likewise skewed.

Pinker correctly reports some of the results of a study by Jenny Singleton and Elissa Newport. Whereas the child's parents, who were the models for the child, made errors 40 percent of the time for certain ASL signs, the child had a 20 percent error rate. Pinker claims that this demonstrates that the child had innate knowledge of the principles underlying ASL else he could not have performed better than his parents. What Pinker fails to note is that the child had a higher error rate than his parents when they incorrectly modeled other ASL signs more than 50 percent of the time. The complete data refute the Chomskian model; the child's behavior is instead consistent with his deriving a "prototype" by means of cognitive principles similar to those modeled in distributed neural networks.

Again, in discussing the genetic bases of syntax Pinker presents Myrna Gopnick's work that supposedly shows that syntax genes exist. Gopnick claims that a single dominant "grammar" gene results in a specific deficit in one part of the UG, sparing cognition, speech and other parts of language. According to Pinker (p. 323), "The K family ... whose members say things like "Carol is cry in the church. ..."is one of the most dramatic demonstrations that deficits in grammatical ability might be inherited." Only part of the story is told. The K family has been studied by Faraneh Varga-Khaden and her colleagues for a number of years; in actuality, the afflicted family members have profound linguistic, cognitive, and speech impairments. Their speech is so distorted and incomprehensible that a BBC film documenting the research project uses English subtitles.

As Vargha-Khadam and her colleagues (1995) demonstrate, the deficits of the K family more likely reflect the impairment of neural mechanisms and pathways that regulate speech motor activity, syntax and cognitive ability. They are consistent with the theory that I proposed in 1985 for the evolution of human syntactic ability, namely that neuromotor mechanisms adapted for manual and speech motor control were the starting point (the preadaptive basis) for natural selection that resulted in the human capacity for learning the complex rules of syntax. I am not alone in believing that general cognitive processes such as imitation and association can account for the way in which children acquire language. Humans appear to have a unique ability to acquire and use the complex rules of syntax, but they also have unique cognitive capacities. Pinker short-changes the opposing data and theories. A comprehensive list of relevant studies that are ignored (parole) would take up a book-length chapter, but the work of Bates, Greenfield, Kimura, Kuhl, Lindblom, MacNeilage, Meltzhof, and Studdert-Kennedy surely should have been noted.

Pinker has an unfortunate tendency to set up straw-men, quoting sentences or sentence fragments out of context. The point of my 1991 book, UNIQUELY HUMAN: THE EVOLUTION OF SPEECH, THOUGHT, AND SELFLESS BEHAVIOR, which Pinker references, is that the neural mechanisms that regulate speech and syntax were key elements in the evolution of modern *Homo Sapiens*. However, Pinker contrives to convince his readers that I deny, let alone have any theory on, the evolution of human syntactic ability. On page 349 Pinker starts by posing the rhetorical question, "How plausible is it that the ancestor to language first appeared after the branch leading to humans split off from the branch leading to chimps. Not very likely, says Philip Lieberman, one of the scientists who believe that vocal tract anatomy and speech control are the only things that were modified in evolution, not a grammar module." Pinker's evidence is a sentence from a 1992 publication in which I argue that human syntactic ability COULD have evolved despite Chomsky's claims to the contrary, The passage from which Pinker excised the middle sentence reads:

> "A strict modular theory of mind is the premise that causes this [Chomsky's] theoretical problem. Since Darwinian natural selection involves small incremental steps that enhance the present function of the specialized module, the evolution of a 'new' module is impossible. Chomsky has not considered the Darwinian mechanism of preadaptation, whereby an 'organ originally constructed for one purpose may be converted into one for a wholly different purpose (Darwin, 1959, p. 190)'."

Pinker's theory (p. 350) for the evolution of syntax isn't very different from mine, he proposes that "Language could have arisen ...by a revamping of primate brain circuits that originally had no role in vocal communication and by the addition of some new ones."

Pinker's most egregious act is his treatment of Beatrix and Allen Gardner. Pinker (p.336) implies that Washoe, the first chimpanzee who acquired the ability to communicate words using manual sign language, never actually signed. Pinker "evidence" is an interview that a disaffected Gardner employee granted to Adrian Neisser in 1979 on the condition of anonymity. Her 1983 book attempted to debunk all aspects of "chimpanzee language." The interview implies that the Gardner's pressured their assistants to record nonexistent ASL signing. The full text in Neisser's book documents the interviewee's anger, it also reveals that he "never even saw Washoe," and wasn't the only deaf person working for the Gardners. Pinker, however, states that he presents the views of "The one deaf native signer on the Washoe team [who] later made these candid remarks." Given the serious nature of this accusation, one would suppose that Pinker made further inquiries before publication. If Pinker had attempted to verify the anonymous accusation, he would have found that 14 deaf ASL and 14 hearing research assistants who were children of deaf parents or had deaf and signing siblings, worked with the Gardners. If Pinker doubted that Washoe had used ASL signs, he could have referred to published papers by disinterested experts on ASL like William Stokoe who conversed with Washoe.

Many other biological claims made in THE LANGUAGE INSTINCT are questionable. For example, on page 327 Pinker claims that genetic "variation from one individual to another must be quantitative and minor; thanks to natural selection, all normal people must be qualitatively the same." Pinker surely is aware of the fact that the "normal" condition that confers resistance to malaria through natural selection, results in sickle-cell anemia? The key word again is "normal," a distinction similar in nature to competence-performance. Pinker accepts the argument advanced by John Tooby and Leda Cosmides that the offspring of mating parents would not be viable if "two people really had different designs." Tooby and Cosmides argue that if genetic variation existed, sexual reproduction would not always result in viable progeny. In fact, Roberts and Lowe in 1975 showed that most human embryos are not viable. Pinker's claim for genetic uniformity is motivated by his defense of Universal Grammar. If "qualitative" genetic variations existed in humans and the acquisition of syntax derived from an innate, genetically transmitted code, than we might expect to find some children who would be unable to acquire some aspect of their native language, but who could acquire other languages.

In short, though the neural mechanisms that underlie human speech and syntactic ability appear to be species-specific, no compelling evidence presently supports the Chomskian model. The level of detail specifying syntax that is genetically coded has yet to be determined.
A more balanced and moderate account would have been appropriate for a book aimed at general readers who are unacquainted with the data base and competing theoretical positions.

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Lieberman's final discussion concerns matters brought up in Mother Tongue 30 (Newsletter) this Winter of 1997-98.

THE *PLANUM TEMPORALE* DOESN'T HAVE ANY APPARENT ROLE IN LANGUAGE.

The argument for the *planunm temporale* (PT) having anything to do with language has always been based on dubious logic. Previous studies have claimed that normal humans have an asymmetric PT while some persons who are slow readers have less asymmetric PT's. But reading is not equal to language. Written texts are a comparatively recent innovation, most of the world's population was illiterate until comparatively recent times, and slow readers usually have no obvious linguistic deficits. Indeed, recent research suggests that slow reading ability can derive from impaired visual scanning and or auditory deficits. Since Patrick Gannon and his colleagues have shown that chimpanzees have asymmetric PTs, the case is closed since chimpanzees don't talk and have rather limited lexical and syntactic ability. No chimpanzee is even close to the linguistic level of a dyslexic reader.

Moreover, the data from studies of aphasia demonstrate that Wernicke's area is not the "seat" of syntax. Broca's syndrome can result in deficits in both the production of syntactically complex utterances and in the comprehension of sentences that have complex syntax. Similar phenomena occur in Parkinson's disease which affects subcortical basal ganglia structures. In fact, permanent aphasia only occurs in the presence of massive subcortical damage. No instance of permanent aphasia has ever been documented for a lesion limited to neocortex

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LANGUAGE EVOLUTION AND EVOLUTIONARY CONTINUITY

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Of the many recent proposals about language evolution, the strongest trend has been towards bridging the ape-human gap without proposing miracles.

Discontinuity theory is not dead, however. Traditional neo-Chomskian theorists of language evolution, of which Pinker & Bloom, and Bickerton, are the best known, have claimed that there are qualitative discontinuities in the evolution of language, and that the mechanisms of Universal Grammar (UG) must be innate, that is, preprogrammed into the infant's brain. Both Bickerton's and Pinker's work have the singular virtue of taking Chomskian linguistics out of the ivory tower and into a public forum where it can be debated in the context of cognition in general. However, their driving idea -- that UG is built in -- is not very credible, because current theories give us no way to implement such a radical innovation within a Darwinian framework (in this, I agree with Chomsky's original stance). A discontinuous emergence of language implies at least one major neural mutation, of an order of complexity quite unlike any other that we know of, that could have established a complex, multilevel "language organ" which would contain a host of UG features such as basic lexical and phrasal categories, X-bar grammars, etc. This places a heavy onus on Cartesians to show what kind of evolutionary mechanism could have supported such a radical innovation. In my view, they have failed to demonstrate either (1) that the UG device exists as a discrete adaptation, or (2) that there is any known means, genetic, neural or otherwise, by which it could have evolved.

The evidence for the modularity of their putative UG device is iffy, to say the least. The "poverty of the stimulus" argument (that children aren't taught language, and that they reconstruct their native tongue from very little evidence) is kaput, a beautiful theory slain by ugly facts (aka empirical research). We now can see, from the work of Peter Juszyck and others, that infants acquire language comprehension skills, including both the prosody and basic grammatical framework of their native tongue, much earlier than formerly believed, and long before they speak. They acquire their production skills later, after a broader intellectual awakening, and in a very rich learning environment.

The so-called "language gene" defense of the UG device is also dead. Gopnik claimed to have found a grammar gene, and Pinker seized upon this notion in his book, The Language Instinct, devoting an entire chapter to it. Since then, the whole episode has turned into something of a scandal about the inappropriate use of data (it turns out that the family in question, has all sorts of cognitive and linguistic dysfunctions, not just a grammar deficit, and that this has been claimed all along by the team of London researchers whose case Gopnik borrowed). Moreover, the claims that Pinker and others have made about Williams' syndrome, and the linguistic savant Christopher, both supposedly giving us living proof that grammar exists in a discrete biological module, are also hard to defend, because in both cases it can be argued that language is neither neatly isolated from other aspects of their cognition, nor set up according to its conventional architecture.

There is another school of thought that deals with language evolution without the UG device assumption. Recently there was a symposium at the AAAS meeting in Philadelphia, including myself, Sue Savage-Rumbaugh, Joan Bybee, and David Armstrong, and some other continuity theorists. There have been many proposals that have tried to circumvent the perils inherent in taking a neo-Chomskian stance on language evolution. In this issue, my own proposals, Terry Deacon's, and Phil Lieberman's have been singled out by ASLIP, but I would remind readers that there are many others involved in this debate.[1] All have tried to maintain evolutionary continuity by retrospectively "morphing" the ape mind into the human mind[2]. I will comment briefly on points of agreement and disagreement.

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[1] Editor's Note: In addition to Deacon, Donald, and Lieberman, Tecumseh Fitch and Stephen Zegura are writing in this issue. We regret that the real contrast could not be shown because M.I.T. linguists/theorists Steven Pinker and Robert Berwick declined to participate, after initially agreeing to do so, because they were "too busy". In Pinker's case he was told quite explicitly that his theory was likely to be criticized or attacked, so if he wished to redress the balance by presenting his own argument and/or writing a rebuttal to those likely attacks, he was welcome to do so. He agreed that he knew these things would happen but he still did not wish to participate in this issue. So be it. Twas not our fault.

[2] Editor's Note: This sentence provokes the additional memory that we had thought to ask Roger Fouts, long time associate of Fifi the Chimp and out-spoken critic of Chomskian and/or neo-Chomskian theory on ape-human cognitive/communicative differences, to write a review. Eventually, we decided it would not be fair to add such a passionate voice to the litany of criticism of Pinker's and Bickerton's work in the absence of their rebuttals. We had not invited Bickerton to participate, partly because we forgot him, and partly because we thought Pinker's considerable eloquence would be sufficient. We are sorry things turned out to be unbalanced. However, that does not mean that the neo-Chomskians would have prevailed, had they participated. Not necessarily!

Terry Deacon's work has a great strength in his argument on the nature of neural epigenesis. He has probably accounted for the neuroplasticity of the language brain better than anyone else. Phil Lieberman, a pioneer in this field, was one of the first to expose the radical and dangerous nature of the human vocomotor adaptation, and the importance of accounting for this adaptation in theories of language origins. I think that Donald (myself) was one of the first (perhaps after Vygotsky) to realize the importance of culture as the driving force in our cognitive evolution, and the importance of changes to our executive brain functions -- such as working memory, attention, and self-monitoring -- in language evolution. Like my colleagues, I have emphasized the primacy of motor evolution in language. Communicative cultures cannot come into existence without first evolving the appropriate production skills.

Although we agree on many key points, there are some points on which I do not agree with either Deacon or Lieberman. For starters, I cannot accept Deacon's adoption of a neo-Peircean[3] definition of symbols. In the Peircean framework, the "symbolic mind" is scaffolded on top of three levels of analysis: iconic, indexical, and symbolic. This is too simple. Symbolic representation could not have evolved in a single leap from a Behavioristic/indexical mind, and cannot be accounted for with a unidimensional system such as Peirce's because it requires too large a neurological leap to bridge from index to symbol. Deacon acknowledges the complexity of this problem, but his adopted schema does not account for the full power and range of human symbolic abilities. He has not even attempted to deal with the array of nonverbal, or analogue, symbolized skills that I classify as "mimetic". This is a major omission, since vocomimetic skill is a specialized channel of mimesis, basic to speech and all human skills, and since it is becoming clear that the concept of mimesis works very well in both a primatological and developmental context. These two literatures have both started to move towards some of my ideas in this regard. In many ways, Deacon's epigenetic argument is a better match to my theory than his own. His proposed epigenetic "takeover" of motor functions by the enlarged human prefrontal cortex is a perfect match to mimesis, not language. Language needs something more (details to be revealed in my next book).

Lieberman's work is a classic case of taking a professional focus -- phonology and speech -- and working it into a much wider theoretical arena.

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[3] Editor's Note: Point of clarification for non-theorists. `Peircean' refers to the sign theory of Charles Peirce, famous American philosopher, forerunner of William James and the Pragmatists. Peircean theory has been extremely influential in ethnological theory; it also appears prominently in Raimo Anttila's introductory textbooks in Historical Linguistics.

Here again, I cannot agree with everything he says. He makes a very great leap from the motor systems to grammar, without filling in the many intervening steps, some of which are not so much motor adaptations as changes in other cognitive fundamentals. The intervening steps are now becoming clearer, thanks to the work of people such as Susan Goldin-Meadow on iconic grammars. Lieberman has also neglected the important nonvocal dimension of language.

There are other areas of disagreement, and these few comments can hardly do justice to the rich debates that currently mark this active area. All of us have probably underestimated the capacities of apes. And we can all agree on at least one thing: natural selection had plenty of raw capacity to sculpt in the Miocene ape, and continuity can probably be preserved in theories of language evolution.

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Innate Mechanisms and the Evolution of Language

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Special nativists like Noam Chomsky and Steven Pinker have advanced four strong claims about the nature of Universal Grammar:

1. It is innate (developing reliably in almost any non-pathological environment),
2. It is unique to humans (there are no homologues in non-human animals, and in particular in nonhuman communication systems).
3. It is universal (shared with no significant variation by all normal humans)
4. It is encapsulated or "modular" (it relies mainly upon functions or mechanisms specific to it, rather than being shared with non-language functions)

The argument most commonly cited to support the claim of innateness has been the "poverty of the stimulus" argument, which is based on the claim that there is no data present in the child's environment rich or explicit enough about the rules underlying grammar for these rules to be learned. Given that grammatical rules cannot be learned, goes the logic, they must be innate. Since even the most extreme nativist would not claim that children come to the world with all the rules of their future language preformed and ready to go, but instead require some period of development during which they are provided with ample exposure to some language, here is what special nativists appear to be claiming: that in the developmental process leading to adult language, the child has access to "innate knowledge" which augments the data coming from the environment in a way crucial to successful language acquisition. The research program for developmental linguists then becomes one of observing all of the input a given child receives, and then calculating what could have been learned given this input. Any skills over and above those are, perforce, innate.

When this argument was first advanced by Chomsky (1965), the nature of genetic material was only beginning to be understood. Watson & Crick had received the Nobel Prize for elucidating the structure of DNA just three years before. The fathers of ethology, Tinbergen and Lorenz, would have to wait another eight years for their Nobel Prize for pioneering the study of animal behavior and instinct from an evolutionary perspective. Given the then-nascent state both of genetics and of neo-Darwinian evolutionary theory, it is understandable that "Aspects of the Theory of Syntax" neglected questions of the evolution of the "innate abilities" that Chomsky claimed make language acquisition possible.

Much has changed since then. We now have an extensive understanding of and control over the mechanisms of inheritance. DNA is sequenced, pored over and manufactured in thousands of laboratories around the world, and we take for granted the ability to splice genes from one organism into another. By 2000 the Human Genome Project will be complete. The prospect of eliminating genetic disease by injecting healthy, genetically-engineered copies of defective genes lies on the near horizon, and the possibility of cloning humans is not much farther off. Important genes underlying the

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development of complex structures such as the limbs and the brain have been isolated and turn out to be shared by humans and fruit flies. In general, our understanding of how genes work and what they code for, while still far from complete, has become truly remarkable, and the accumulation of knowledge shows no signs of decelerating.

Meanwhile, progress in understanding evolution has been likewise impressive, particularly in the area of behavior. We have learned a great deal about our hominid ancestors, about the communication systems of other mammals, and about the behavior of our nearest animal cousins, the great apes. A number of apes have been trained to communicate using hand gestures borrowed from human sign language, demonstrating abilities that are far beyond what researchers in the 50's had imagined possible. Vervets have been found to have different types of alarm calls which appear to signify different types of predators. Great progress has been made in understanding the neural bases of behavior both in animals and man, and in elucidating their similarities and differences among species. We are now in a position to discuss the genetic information which would underlie "innate" abilities or knowledge, to understand the neural mechanisms underlying such abilities, and to understand how such mechanisms could have evolved. In short, the time is ripe to begin the project of reconciling the views of special nativism with modern biology and evolutionary theory.

Stephen Pinker's 1994 book "The Language Instinct" can be seen as an attempt to do just that. Despite the fact that the result has some of the characteristics of a shotgun marriage, I think he deserves wholehearted congratulations for producing a book which is eloquent, engaging and often downright fun. Pinker has a knack for examples and a way with words that will reliably elicit the admiration and envy of every writer who has ever tried his or her hand at a popular book. Most importantly, though, the book tries to finish the job that Chomsky left unfinished: fleshing out the evolutionary story which must underlie any "innate" mechanism, in particular the putative innate abilities underlying language acquisition. For doing this in such an accessible way, we researchers interested in the biological foundations of language owe Pinker our thanks.

This said, it is the purpose of this article to point out several ways in which Pinker's account fails at the goal of combining the evolutionary and nativist views. In particular, I will argue that the addition of the evolutionary perspective invalidates the simple logic of "poverty of the stimulus" arguments, and instead demands that we balance "learnability" constraints on the one hand with "evolvability" constraints on the other. Second, a trait which is both universal and uniform (that is, genetically fixated) and species-unique is the most difficult type of trait to evolve, and this difficulty is exacerbated in a species like *Homo sapiens* which has diverged very recently (in evolutionary terms) from the ancestor we share with apes. Third, notions of "modularity" or "encapsulation" typical of special nativist thought, which suggest that the functions underlying language are unique to language, create even greater problems, especially given the extremely abstract nature of the "innate" abilities that linguists are interested in. Finally, there is no valid evidence currently available supporting the notions of specific "grammar genes" (despite Pinker's flawed evidence to the contrary), and an ever-increasing body of

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evidence pointing to the opposite conclusion: that the genes coding for brains and parts of brains have been remarkably conservative over evolutionary time, and the phylogenetic story of the human brain is probably one of creative use of pre-existing components, not the innovation of completely new ones. In short, the rigorous adoption of an evolutionary perspective undercuts or at least renders tenuous all the claims of special nativism as applied to the putative "language instinct".

Most contemporary nativist writers, including Pinker, are well aware of the pitfalls of a naive nativism familiar from the popular press - the simplistic formulation of the "nature/nurture question". To ask whether a certain characteristic is "innate" ("genetically determined" vs. "environmentally determined") is to ignore the fact that any biological trait requires (and is affected by) both genes and environment: neither is operative absent the other. This recognized, there is nothing intrinsically disreputable about the nativist stance. The nativist research programme can be taken as a shorthand for the question: "to what degree and in what ways does a behavior vary as a function of genetic variation on the one hand vs. environmental variation on the other?"

Evolution and Learnability

The logic of the "poverty of the stimulus" argument relies on the assumption that "that which is not learned must be innate". If we knew what is learnable, that is, how the learning system works and what its input is, we could subtract this component from the observed behavior of language, and the residue is what is innate. The problem is that we neither know how the learning system works, nor what its actual input is. No one supposes, for example, that the child's input is restricted to a series of sentences that we can write down. This would ignore the incredibly rich environmental and social context in which such sentences are uttered, as well as the prosodic information that is part and parcel of speech. While we could audiotape all of the utterances a child is exposed to, we would still confront a problem of analysis: how do we know which cues might be used by the child and which ignored? Though we might pick out obvious cues such as pitch, there are many more aspects of the voice that provide useful cues for adults. Thus our analysis of the child's verbal input would always be at risk of omitting potentially valuable data. A similar and more severe problem dogs any attempt to record the environmental and social context of the child: even if we had 360° video gear mounted on the child's head to continuously record its context, we wouldn't know where to begin in condensing such a huge volume of data into those pertinent for the developing child. In conclusion, specifying a child's "input" with any degree of accuracy is not a trivial problem.

Second, our knowledge about how children learn anything (not just language) is still hopelessly incomplete. No one would seriously claim that we can comprehensively describe any learning mechanism, let alone that underlying a process as complex and prolonged as language acquisition. Thus, the notion that we can exactly specify what is learnable, a prerequisite of the poverty of the stimulus argument, can be seen to be hopelessly optimistic. Of course, we do know some things about learning, and we can make some educated guesses about those aspects of the environmental and social input that are likely to matter, but we can't specify them precisely and can't be

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sure we haven't overlooked some crucial aspects of the input. This suggests that the state of the art in "learnability theory", while useful, only provides one side of the argument for understanding innateness.

Fortunately, this is precisely where the biological perspective comes in. For something to be innate, it must be in some way coded in our genetic material. Darwinian evolution is the only non-magic mechanism currently known by which complex mechanisms can come to be coded in DNA. Thus, a claim that a complex behavior such as language acquisition is innate is tantamount to the claim that it has evolved through a process of natural and/or sexual selection operating on random mutations over large periods of time. I say "complex behavior" to acknowledge the fact that other mechanisms besides Darwinian selection do operate in phylogeny, such as genetic drift. However, such factors cannot account for highly complex structures such as eyes or language instincts (as Richard Dawkins explains with great eloquence in his "Blind Watchmaker").

This suggests that the necessary complement to "learnability" issues are considerations of "evolvability". As in the case of learning, our knowledge of evolution is far from complete, and requires not just theoretical models of evolution but detailed knowledge of the genetics and development of the traits under study. Thus, both learnability and evolvability should properly act as constraints on hypothesis generation: if the learnability of a task seems extremely low, and evolvability high, the hypothesis of innateness becomes highly plausible. To take an uncontroversial example, if a squirrel monkey is raised by a muted mother in isolation from conspecifics and is never exposed to the sounds of its species-typical repertoire, it will nonetheless produce normal exemplars of those sounds, in more or less normal behavioral contexts (Winter et al. 1973). First, the absence of appreciable vocal input greatly reduces the chances that these vocalizations could be learned. Second, the evolvability of this system appears to be quite high, because many of the monkey's calls function in mother-infant communication, and would be most effective if present at birth. Furthermore, the sounds used are quite similar to those seen in many other primates (e.g. a long frequency-modulated "peep" emitted when separated from the mother or group, often termed the "isolation call"), as well as other mammals, suggesting an ancient phylogenetic lineage and requiring tens of millions of years to evolve and stabilize genetically. Thus, both evolutionary and learnability considerations point to the same conclusion: squirrel monkey calls are to a large degree innate, in the sense that they reliably develop in the absence of any relevant environmental input. This example shows that the combination of learnability and evolvability perspectives allows a richer analysis of a trait than one based on learnability alone. I now turn to analyzing the claims of special nativism from this point of view.

Uniqueness and Universality

Returning to special nativism, let us examine the two claims that the "language acquisition device" or LAD is both unique to humans, without homology in other organisms, and is the same in all non-pathological humans. In biological terms this means that 1) the set of interconnected mutations and/or gene duplications that together comprise the putative LAD occurred in our hominid ancestors at or after our split from the great

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apes, our closest living relatives, and 2) that this gene complex has gone to fixation since that time, that is, that alternative alleles have been eliminated by selection. While there is nothing to make this combination of assertions intrinsically invalid, it is important to note that together these claims decrease the evolvability of the putative LAD. To see why, notice that recently-evolved traits typically show high initial variability which takes many generations of strong stabilizing selection to reduce. In the case of recessive alleles, which can only be subjected to selection when they appear in the homozygous state, even powerful selection will be ineffective at reducing the allele's frequency once it becomes rare, and its frequency may never reach zero (it will only approach it at an asymptotic rate). Thus, experiments in artificial breeding have shown that even after long periods under extremely powerful selection regimes, which dwarf anything seen in nature, residual genetic variability is still very high: after 50 generations of powerful selection for high protein content in corn, the plants retained sufficient genetic variability to return rapidly to baseline when reverse selection was enforced (Dudley 1977). In general it takes a very long period of very powerful selection for a dominant allele to go fully to fixation.

The converse of this is also true: the traits lowest in genetic variability tend to be the most ancient. The chemical structure of nucleic acids, lipids, sugars and most proteins which comprise the body, the basic metabolic pathways which run it, and the workings of genetic code itself are all typically fixed and identical in members of a given species. But they are also among the most widespread features of living things, essentially constant throughout vertebrates and, in the case of the genetic code, throughout all life on our planet. Thus, when biologists see fixation, they have come to expect phylogenetic depth, and thus that the trait in question will be shared with related species. While neither fixation nor uniqueness is unusual alone, their combination in a recently-evolved species would require extraordinary circumstances.

Given the ubiquity of genetic, and thus morphological, variation typical of innate traits, the claim of "uniform universality" might seem somewhat naive. But a widespread mistaken assumption allows special nativists to duck this charge: that despite "quantitative" genetic variability, innate traits are "qualitatively uniform" from a functional point of view. Quoting Pinker (p. 326):

> It is easy to get carried away with the geneticists' discovery that many of our genes are as distinctive as our fingerprints. After all, you can open up any page of Gray's Anatomy and expect to find a depiction of organs and their parts and arrangements that will be true of any normal person.

But a closer inspection of Gray's Anatomy reveals it to be full of descriptions of "peculiarities" in the anatomy of different organs, in particular for the circulatory system which Pinker later cites as being so constant. The study of such variant structures makes up an important component of the study of anatomy, and has important practical ramifications, for example for surgeons. Such disorders as heart murmurs, found in many "normal" and fully-functional humans, result from differences

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in anatomy which are a commonplace aspect of human form, just like height and hair color differences. In my recent personal experience examining hundreds of human skulls, it is quite astounding the degree to which they vary, including significant differences in number and shape of bones, locations of sutures, structures of muscle attachments and presence or absence of whole functional structures like the exo-occipital protuberance. Similarly, human brains are notoriously variable in form (and probably function), and in general all innate traits currently known show significant variation between individuals. In fact, this sort of within-species variation is the fodder for evolution itself: without functional variability between individuals, natural selection would have nothing to select. Thus, individual variability is such a commonplace for the biologist that it typically evokes little but a footnote.

Pinker's answer to such data is to acknowledge the possibility of some variability in innate language modules, while asserting the functional equivalence of such variants. While "the complexity of language circuitry leaves plenty of room for variation", Pinker (p. 329) proposes that the "basic design of language, from X-bar syntax to phonological rules and vocabulary structure" is "uniform across the species; how else could children learn to talk and adults understand one another?". While the last phrase of course assumes what nativists want to prove, Pinker provides a stronger argument courtesy John Tooby and Leda Cosmides (1990) who claim that differences between individuals of the same species MUST be minor quantitative differences, because qualitatively different organisms could not produce viable offspring. To explain why, Pinker uses the colorful metaphor of the blueprints for two "parents", a Ferrari and a Jeep, being cut into small pieces and then randomly taped together. "The resulting contraption, if it could be built at all, would certainly not get anywhere", he concludes. This argument ignores organisms such as the deep sea fish *Photocorynus* in which males are tiny parasites lacking digestive systems, which attach permanently to the much larger females. Less extreme dimorphisms are commonplace in biology, and do not interfere with the ability of these very different mates to reproduce[1]. This is because genes are not "blueprints" of a finished body plan, but rather "recipes" for steering a developing organism through the epigenetic landscape. These recipes are highly buffered and resistant to catastrophic disruption, and thus quite tolerant of even qualitative variations between mating individuals. Such epigenetic buffering is strikingly revealed in studies in which researchers remove entire genes (often ones thought to play key roles in important metabolic or developmental pathways) from an individual mouse. In some cases, the resulting offspring (called "knockout" mice) are normal: apparently alternative developmental and metabolic pathways can pick up the slack. In summary, there is no practical or logical necessity for differences between mates to be quantitative, or for species to be "qualitatively uniform".

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[1] To be fair, Tooby and Cosmides (1990) do address this issue, redefining "qualitatively different" organisms as "morphs" which they claim are controlled by binary "switches" like the Y chromosome in mammals. But in fish, sex determination is polygenic and not binary, and there are many examples of non-pathological males which look and act like females and vice versa. In any case, their discussion of this point is a definitional attempt to have their cake and eat it too, since it admits the existence of qualitative variability within a species.

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A different way around the observation that extraordinary circumstances are required to get fixation and uniqueness is to suggest that human evolution involved just such circumstances. An example might be genetic bottlenecks caused by "founder effects". When a new species is created from a small population of founder individuals, the effect of random drift is much greater than for a larger population, and drift can thus aid in the elimination of rare alleles. This has been demonstrated for the flour beetle *Tribolium* by Rich et al. (1979), who showed that populations started with ten beetles sometimes reached fixation rapidly while larger populations did not reach fixation at all. For humans the argument might go: an early, small population of our ancestral hominids developed the gene complex underlying the LAD, which went to fixation in their small population and then spread (à la the "out of Africa" scenario) to conquer the world. Ignoring for the moment the fact that this hypothesis cannot account for the elimination of mutations accrued during the subsequent population expansion, we might ask how well it fits with what is known about human evolution.

Habitual bipedalism, another uniquely human characteristic (among primates), provides a revealing comparison. It is a recently evolved trait (developing since our divergence with chimpanzees about 5 million years ago) which was essentially complete in early australopithecines like "Lucy", who, despite her small brain, had many of the modifications of the spine, pelvis, leg and foot which are associated with bipedal posture in modern humans. Bipedalism is also universal in humans. But is it functionally uniform in all humans? Clearly not. We suffer from a wide variety of common ailments associated with uprightness, including fallen arches, bum knees, afflicted hips and slipped disks, which together demonstrate a considerable variability in the genes specifying locomotory anatomy and physiology from person to person. Many of these problems can arise quite early in life, and cannot be explained away by the fact that contemporary humans outlive the typical life span of our ancestors. This is precisely what we would expect given the facts: bipedalism is recently evolved and unique to our species, and a few million years has not been enough time to work the kinks out of the system and drive it to fixation. Presumably selection pressures for effective walking have been consistent and extremely powerful for the last three million years (that is, at least since "Lucy"), but neither this nor the possibility of founder effects have been sufficient to perfect this system and drive it to fixation. Since the time span for perfecting the LAD in modern humans is probably even shorter, with some estimates of as little as 150,000 years, we may ask how plausible it is that selection on the LAD has been much stronger than selection on our ability to walk.

In summary, combining the claim of uniform universality with that of uniqueness gives the proposed LAD a low "evolvability score". This observation does not by itself render the combined claims of uniqueness and invariance (or either one alone) impossible. It does show that the evolvability of a hypothesized "language instinct" would be increased by either allowing it to vary significantly among humans, or by allowing it to share characteristics with instincts in other animals. Therefore, before accepting nativist claims of uniqueness combined with invariance, we might

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demand more convincing evidence than would be required if either claim were made alone.

Pleiotropy and Modularity

The next claim is that the innate abilities underlying language are domain-specific or "modular", performing tasks specific to language and not shared with other faculties (such as general problem-solving ability, social behavior, or acquisition of non-linguistic aspects of culture such as gestures, dances, music, etc.). It is here that special nativists like Pinker (along with "evolutionary psychologists" like Tooby & Cosmides) run into bigger problems. Genes are modified individually by mutations, but selection acts on entire organisms. To the extent that a given gene has a variety of effects on many aspects of the organism, and these effects all modify fitness in a consistent way, it can be easily favored by selection. Indeed, one appeal of the notion of general intelligence stems from the fact that it would prove useful in almost any phenotype and any environmental situation. A gene which has a positive effect on feeding efficiency, courtship success and predator avoidance clearly has more going for it than one which affects just one of these (or two, but in different directions).

Empirically, all of the genes currently understood have diverse effects on the organism as a whole, and changes in one gene typically lead to a bewildering variety of physiological, morphological and often behavioral effects. A good example is the recently-cloned cystic fibrosis gene, which codes for a single membrane-bound ion channel. Individuals with a mutant CF gene show a diverse array of respiratory and digestive problems, unusually salty sweat and an atypical "clubbed" fingertip morphology. This one-gene, multiple-action phenomenon, traditionally termed "pleiotropy", is the rule in genetics, not the exception.

In this context it is worth mentioning the supposed "grammar genes" discussed by Pinker at various spots in his book, which would appear to be exceptions to this rule. The claim of a single autosomal gene specifically affecting morphology and syntax was based on work by Myrna Gopnik (1990) on several members of a family which shared a defect that Gopnik claimed was specific to grammatical rules. As was known at the time, and has since been reiterated by Vargha-Khadem et al. (1990, 1995) who worked with the same family, these unfortunate individuals suffer from a much more general syndrome including severe disruption of their oral and facial motor skill, allied deficits in speech, significantly lower IQ and memory scores than unaffected members of the family, and deficits in all language skills, not just morphosyntactic deficits. Thus Gopnik's (and by extension Pinker's) claim that this family provides evidence for distinct "grammar genes" is invalid.

Combining the claims of innateness, modularity and universality, the result is the classic "innate module" that has become the mainstay of both special nativists in linguistics and "evolutionary psychologists" in psychology. The basic intuition underlying its existence is concisely summarized by Cosmides & Tooby (1994, p. 89): "a jack of all trades is necessarily a master of none". Using Chomsky's "organ" metaphor for behavioral adaptations, they (and Pinker) argue that there is no better reason to

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expect "domain general" organs in the brain (e.g., a general problem-solving ability) than there is to expect that the eye and heart could be replaced by an organ which both senses light and pumps blood. While there is certainly some truth to the notion that selection can act to increase specialization (extraordinary degrees of ecological and behavioral specialization are found in organisms as diverse as anteaters and liver flukes), such specialization is dependent on long periods of essentially constant selective pressures, as for organisms living exclusively in one particularly unvarying habitat. When the environment changes, such highly specialized organisms are often the first to go .

As usual in nature, diversity is the rule. In contrast to highly specialized organisms like anteaters, there are highly generalized organisms like ravens and crows (corvids), which evolved recently in Australia/New Guinea and have since spread around the world, occupying an impressive variety of habitats including rainforest, tropical savanna, mountainous regions and tundra. Corvids are thought by many ornithologists to be the most intelligent birds. They are omnivorous, highly curious, easily outsmart mammals such as cats, readily exhibit one-trial learning and food caching. They are highly social with extended juvenile periods and cooperative breeding systems including juveniles as "helpers at the nest" (Kilham 1995). For such organisms, domain-specific abilities are less useful than an ability to quickly and effectively learn to exploit cues in the environment which may be completely novel and thus unexploitable by the long, slow process of random mutation and powerful selection that would be required to build innate modules. For example, the ability of ravens to recognize the bamboo stakes used by mountain climbers to mark food caches (Heinrich 1989) is precisely the sort of task best mastered by a general learning mechanism.

Such behavioral flexibility is quite common among vertebrate species. For example, the bluehead wrasse, *Thalassoma bifasciatum*, occupies virtually all of the available ecosystems in the Caribbean (coral reef, tidal flat, mangrove swamp, eelgrass beds, etc.) and is a consummate generalist: it can adopt different social systems and even change sex according to the situation at hand (Fitch & Shapiro 1990). Studies by Warner (1988) and colleagues have demonstrated "cultural" transmission of information in the same fish, where experimentally induced "traditions" are passed down for generations. These examples, which could be endlessly multiplied, suggest that there are big advantages to flexible, domain-general mechanisms if the environment is highly variable, advantages that nativists seem to have ignored.

It is clear that humans are generalists when it comes to habitat and feeding, and it seems unlikely that humans fall into the hard-wired specialist category when it comes to behavior either. Many aspects of our diet, social life, mating behavior and kin-related behavior are clearly culturally determined, and this probably allows groups of humans the flexibility to learn from the environment and to share that knowledge with one another, an ability that plays a substantial role in the modern success of our species. To quote Robert Heinlein, "specialization is for insects". The claims of researchers like Tooby and Cosmides as to the automatic

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superiority of specialized mechanisms, which Pinker appears to accept, are in fact oversimplified and readily falsifiable.

Conclusion: Unifying the Nativist and Evolutionary Viewpoints

In my opinion a much more reasonable case can be made that the abilities comprising a "language instinct" form a mosaic, most of which are homologous to abilities found in our animal kin. Abilities such as associative memory, selective attention, sophisticated auditory processing, and motor automatization play an important role in the mastery of language, and appear to differ very little between humans and other mammals. This is the evolutionary view long championed by Philip Lieberman (e.g., 1975, 1984, 1998). Other abilities such as an innate desire for social conformity may play an important role in learning and are found in primates but not all mammals. Finally, the human ability to learn arbitrary vocal behaviors and mimic sounds, while not seen in non-human primates, is commonplace in birds. Given the highly conservative nature of neurophysiology and the developmental genetics underlying nervous structure, it is not implausible that versions of the same mechanisms are responsible (though this may stretch the term "homology" past the breaking point). If true, this is good news for those interested in understanding these mechanisms, because it means that studies of animal communication, cognition, neurophysiology and genetics can provide valuable information about the evolution and functioning of human language.

As for "uniquely human" traits, it may be that there are some aspects of human developmental genetics that are specific to humans (perhaps some which arose via gene duplication and copy modification, as has been observed in many other systems). If so, we expect these to possess significant variation, so that studies of language impairments like specific language impairment or dyslexia might help locate them, as Pinker acknowledges. But even non-modular deficits such as those misrepresented by Gopnik and studied by Vargha-Khadem can provide valuable insights into the genetic basis of language: a language deficit does not need to be modular for it to be enlightening. To my knowledge, for example, no one has attempted a comprehensive study of the syntax of the severely retarded. Taking a broad viewpoint and using all of the data available from evolutionary theory, human variability, linguistic theory and developmental genetics, while challenging, seems to be the most promising way to triangulate on what is truly unique about human language. Attempts like that of Terrence Deacon's in his (1996) "Symbolic Species" offer a constructive step in this direction. It seems likely that is the direction in which biological linguistics will progress in the next century.

In summary, by accepting the four fundamental claims of special nativism while adopting an explicitly neo-Darwinian viewpoint, Pinker paints himself into a corner. While no one of the claims is by itself particularly implausible, their combination is. Of course, biology is full of examples of things which would certainly seem implausible: one might understandably be skeptical if told of an orchid which ensured pollination by inviting the amorous intentions of male wasps. Thus, it could indeed be true that there are neural mechanisms which play a role exclusively in language, and are coded for by genes bearing no relationship to those found in our nearest animal relatives, which furthermore show no significant variation among

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individual humans. But given the tenuous (essentially rhetorical) nature of the evidence which has so far been offered by nativists, a skeptic might be forgiven if he or she is not yet convinced that this is in fact the case.

The nature of language excites powerful emotions in those who study it, much more so than, say, the nature of electrical communication in fish does in ichthyologists. This is probably because our ideas about language have much in common with our ideas about our own deepest nature. Unfortunately, the field has been characterized by misunderstanding, vicious infighting and unscholarly disrespect towards those with opposing ideas. In a spirit more appropriate to a high-school debating team than a mature scholarly discipline, critics have too often attempted to make ideas they disagree with appear to be utter nonsense. Quoting Susanne Langer (1962) "the chance that the key ideas of any professional scholar's work are pure nonsense is small; much greater the chance that a devastating refutation is based on a superficial reading or even a distorted one, subconsciously twisted by a desire to refute". Thus, I would like to end with a plea extended to both sides of the nativist debate as it pertains to the evolution of language: in a field as complex and multidisciplinary as biological linguistics, surely we all have something to gain by both giving and receiving constructive criticism in a spirit of scholarly camaraderie. That is certainly the spirit in which the current critique is offered.

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1. RECENT COMPASS POINTS IN THE EVOLUTION OF LANGUAGE DEBATE

Terrence W. Deacon, Boston University, (March 1998)

Introduction

If one measure of the maturation of a field of study is how completely the set of alternative positions have been articulated and assessed with respect to each other, then the study of language origins may be approaching adolescence. It's long infancy has included the fantasies of early armchair speculations, the taint of illegitimacy, banishment from serious linguistic study, subordination to the study of intelligence, and finally its unavoidable reappearance in the questions begged by linguists, developmental psychologists, human paleontologists, neurologists, and animal behaviorists. Just in the last few decades we have progressed from a field dominated by vague and nonempirical scenarios about how language might have begun to a diverse multitude of theories, more firmly rooted in one empirical field or another.

Some of the best evidence for the maturation of this field can be found in the recent publication of numerous books, originating from quite different fields, each adding crucial new insights. This is not evidence of progress toward a convergence of opinion, however, even though many popular accounts of recent advances in the cognitive sciences have suggested a kind of grand synthesis may be forming. If anything, the lines of theoretical antithesis are now more sharply drawn than ever before. But, as a result, the crucial problems have been more clearly exposed and dissected. This diversity offers us an opportunity to take stock of problems that are shared by all, and to recognize issues that may have been swept under the rug of dominant academic fashion.

Though I am a partisan in the current debate, I will try to stand back and describe this diversity first before attempting to dissect away what I assess to be the less useful lines of inquiry and point the way toward an empirical science of language origins.

In this brief review I will contrast ideas presented in my own recent book, The Symbolic Species (TD)[1], with those in a few other recent books:The Language Instinct by Steven Pinker (SP), Origins of the Modern Mind by Merlin Donald (MD), andEve Spoke by Philip Lieberman (PL). Below I also offer a few comments regarding some previous books on the subject, including Language and Species by Derek Bickerton (DB) and The Evolution of Communication, by Marc Hauser (MH). A summary of this comparison is presented in Table 1 and outlines some of the major topics each addresses.

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[1] For simplicity I will refer to the major players in this intellectual debate by their initials and will include reference to my own views also in this form in third person in order to avoid confusion and ambiguity.

These books roughly span the compass points of the current state of language origins research. So to begin, let me attempt an overview of the major positions in the field with the following table:

[insert Table 1]

Surveying this multidimensional array it should become apparent that there are not just two opposed perspectives and that views considered opposed in one conceptual dimension are not always opposed along others.

What is unique about language?

All the authors begin with the view that language is unique to humans, but they differ in how they describe the nature and importance of this uniqueness.

At one end of the spectrum are SP and DB, who assert that language is a biological "organ" that has no evolutionary precedent or precursor. More specifically, they claim that knowledge of the syntactic and grammatical logic of language are unprecedented additions to human cognition. This reflects a dominant assumption among linguists: that language evolved with the evolution of an innate modular language faculty (IMLF) or universal grammar (UG).

At the other end of the spectrum are PL and MH who argue that there is a continuum of abilities and functions linking human and nonhuman modes of communication. For them, language is unique in the provisional sense that most species' adaptations are unique: different in degree and in combination with other adaptations. Language is described as merely a more complex and efficient form of communication, supported by increased intelligence and sensorimotor specializations. In other words, that the critical differences are mostly in the expression and reception of language rather than the thought processes behind it.

In both of these opposed views, the uniqueness of symbolic reference is downplayed or ignored. Instead, the focus is on abstract complexity, either UG, general intelligence, or speech specialization. This leads each to suggest comparisons between animal calls and words or holophrastic speech lacking syntax. For example, both proponents of an IMLF argue that there could have been something like a protolanguage in early human evolution that lacked grammar and syntax. Such a protolanguage is explicitly compared to toddler-stage language, Broca's aphasic speech, or vervet monkey alarm calls. Continuity theories make a similar assumption. The differences between the IMLF and continuity views is mostly over whether general learning mechanisms and increased intelligence are sufficient to account for human language acquisition and the rules of UG. The lack of distinction between forms of reference in MH and PL is exemplified by their suggestions that were it not for the limited vocal abilities of nonhuman apes, they would probably be capable of a considerable level of language use and human cognition. Thus MH and PL assume that a referential and a syntactic continuum links animal and human communication, whereas SP and DB assume that there is a referential continuum but that UG arose independently without such precedent as an add-on to more typical primate vocal calls and thought processes..

It is on this point that The Symbolic Species diverges from the other approaches. Curiously, despite a critical view of UG assumptions, TD sides with the linguists (SP and DB) on the discontinuity question, at least in terms of semiotic function. But this turns out to be for entirely different and orthogonal reasons. TD disagrees with one assumption common to both extremes of the above arguments: that the underlying mode of reference and conceptualization is the same for language and nonlanguage communication and thought. It is not just syntax that is distinctive, but the mode of reference of language. Specifically TD argues that grammatical categories and the systemic nature of syntactic operations in language are derivative features of the symbolic mode of reference and not an essential and independent feature of language. This is implicit in TD's conception of the symbolic relationship: indirect or virtual reference in which the combinatorial and substitutional relationships between signs (here referring generically to the physical tokens themselves) mediates reference to things in the world (as opposed to being mediated by the correlation and simple co-occurrence of signs and referents). Thus, TD agrees in principle with SP and DB but argues that their assessment of the locus of this difference is functionally too superficial: animal communication does lack grammar and syntax but only because their communication is nonsymbolic.

TD treats symbolic reference as the core feature of language. He argues that neither the difficulty of learning the structure of language or using such a huge database of sounds and associations can explain why only humans have language. Not even simplified language systems are found in other species. So the complexity of language cannot be the problem. The problem is its form of reference: symbolic reference. He traces this difference to cognitive predispositions that work against symbol learning in most species. In answering the question how this was overcome, however, it reverses the basic evolutionary logic found in each of the other texts. Rather that postulating changes in the brain or vocal tract that preceded or allowed language, it argues that the use of symbolic communication preceded the evolution of uniquely human cognitive abilities and was directly involved in their appearance. The basic claim is that symbolic communication itself selected for cognitive adaptations that make it easier to acquire and use. This inversion is made clear by one of the chapter titles: "And The Word Became Flesh." PL explicitly argues that the cognitive apparatus in other great apes, includes language-like thought and syntactic-like analysis abilities, and that their major limitations are primarily in the facile expression of these mental contents in the form of speech. I interpret PL to imply that the evolution of articulate abilities amplified these cognitive abilities by providing a powerful tool in the form of a readily available sensorimotor substrate to anchor and organize such cognitive processes. This seems to be the similar to the position argued by MD, who considers the lack of articulate speech as a major factor contributing to an intermediate mimetic stage in human evolution, between nonlinguistic ancestors and ourselves.

But this is a crucial difference because it leads TD to a deeper continuity argument that ultimately puts him closer to MH and PL. He argues that symbolic ability is not the result of any novel anatomical substrate, but rather supported by a shift along a continuum of pre-existing component cognitive capacities found also in other species: descent with modification. More to the point, he suggests that there is a sort of critical learning threshold (discussed below), blocking access to symbolic referential abilities, that is far more easily surmounted in humans than in any other species because of differences in what might be called a cognitive strategy correlated with our shift in component neurological resources. For TD, symbolic communication and implied grammar and syntax in some form are not in principle considered to be beyond the capacity of many other species, under the right circumstances. TD leaves open the possibility that syntactic knowledge might be cognitively accessible to many other species if they could learn to communicate symbolically.

What is the role of general intelligence?

Though one might expect there to be little disagreement over the question of whether an increase in intelligence characterizes our species with respect to others (assumed to be associated with increased brain size, but see below), there are clear differences of opinion among these researchers as to its role, if any, in language evolution. The basic divergence of opinion can again be characterized as distinguishing the linguists from the rest. SP and DB have little to say about general intelligence, and by implication brain size in humans. It is essentially irrelevant to the IMLF view, except in so far as a minimum intelligence might be a prerequisite in order for a language faculty to be useful. Almost nothing is said about the evolution of intelligence by DB and SP, except in so far as they assume that comparatively high intelligence is a minimal precondition for language, though not sufficient in itself.

In contrast, biologists, evolutionary anthropologists, and archeologists ubiquitously cite the evolution of increased intelligence as one of the hallmarks of our species. Though I would argue that this is an assumption based on a fairly weak foundation of empirical testing of interspecific intelligence, it has considerable intuitive appeal and has essentially achieved the status of an unquestioned truth in popular accounts. It is generally included as a key element in most continuity theories of language evolution. A general learning approach to language origins and language acquisition has also provided the principle antagonist to IMLF theories, and the major debates about the nature of language abilities have often been framed as an either/or choice between these alternatives.

PL and MD each imply that an increase in intelligence in human prehistory has played a critical role in language evolution. Language is treated as the expression of this. However, unlike many prior general intelligence theories of human evolution, neither suggests that an increase in intelligence was in itself sufficient to explain the origins of language. Both PL and MD argue that language appeared only after a period of independent selection for increased intelligence, implying that it may have served as a necessary but not sufficient prerequisite.

Though antagonistic to IMLF theories, TD also devotes two chapters to debunking and replacing what he sees to be simplistic arguments for a correlation between brain size (or encephalization) and general intelligence. He criticizes this on two grounds: 1) a re-examination of confusions over implicit assumptions about gross as opposed to net information processing measures, and 2) a demonstration that past allometric assessments of encephalization have confused at least three distinct processes that predict very different information processing outcomes. This sets the stage for his alternative componential analysis of brain size change in evolution, which stresses the central role of trade-offs in the development of connections and biases in processing strategies.

Origins of vocal abilities

Claims about the origins of articulate speech with respect to other threads of the human evolutionary scenario also strongly divide these theorists. The critical adaptation that, according to PL and MD, allowed the transition from an inarticulate but intelligent ape to an ape possessed with language was a reorganization of the vocal tract and vocal control system making articulate speech possible. This leads these writers to postulate a distinctively biphasic theory of human cognitive evolution, that includes an early phase of increasing general intelligence and a much later phase in which speech appears. This hypothesis has become particularly popular among archeologists who see it as an explanation for the relatively recent (in paleontological terms) explosion of cave art, tool diversity, adornments, etc. Though PL's earlier works suggested that articulate vocal capabilities were confined to anatomically modern humans, he now appears to be suggesting a somewhat more continuous and more protracted evolution. Nevertheless, his book title, Eve Spoke, exemplifies his emphasis on the special role he sees speech playing in the recent origins of anatomically modern humans. The evidence about this transition suggests to him that the apparent rapid expansion of anatomically modern Homo sapiens out of Africa to displace all prior hominid populations in other parts of the world was facilitated by an advance in the production of speech. Following others (e.g. , the late Alan Wilson, one of the originators to the mitochondrial Eve hypothesis), he suggests that all modern humans trace their mitochondrial gene ancestry to a single common female alive about 100,000 years ago, because she had some special mutation conveying essentially modern vocal abilities. What is not made clear in this account, however, is that mitochondrial genes cannot contribute to this directly (only to oxidative metabolism in general) and that the nuclear genes that must have contributed were inevitably inherited from a much much larger sample of contemporaneous ancestors (from a pool of as many as 100,000 individuals according to estimates based on MHC gene diversity). Eve was not special in any other regards except as a marker for the founder for one fractional lineage of modern genes.

An even more dichotomous and quite explicitly biphasic scenario for the evolution of speech is proposed by MD. This scenario follows a long tradition of theorists (e.g. see contemporary reviews by Gordon Hewes) who postulate a discrete intermediate stage in language evolution characterized by the dominance of manual signing and other forms of nonverbal communication. MD postulates that Homo erectus' social communication may have been a kind of mimicry-based communication, that he claims to be the basis for an entire mimetic cultural adaptation. He introduces this discrete intermediate stage to suggest an origin for human esthetic and ritualistic predispositions that is independent of language, to postulates a correlation between a unique and distinct cultural adaptation and the erectus paleospecies distinction, and to bridge the gap between nonverbal ancestors and ourselves, suggesting a kind of punctuated equilibrium as opposed to continuous modification.

In contrast, TD argues that the last 2 million years of hominid evolution was characterized by a relatively continuous trend toward increased development and use of speech. He offers a neuroanatomical theory to account for the very limited vocal capabilities of nearly all mammals. In this group he includes australopithecines and members of early Homo. He thus agrees that the earliest language-like communication was probably mostly nonverbal, but he also provides neurological evidence to suggest that this condition was continually changing as a direct consequence of increasing encephalization. This would suggest that vocal abilities gradually improved beginning with Homo habilis and culminating in Homo sapiens (with a tempo correlating with encephalization). PL and MD (following PL) argue that the descent of the larynx to its anomalously low position in the modern human throat is an index of vocal ability. The apparently late evolutionary achievement of this state suggests that speech was not present or at most minimal before anatomically modern Homo sapiens. TD argues instead that laryngeal descent lagged considerably behind increased motor control of tongue and laryngeal muscles because it would only have been selectively favored in a context of dependency on articulate vocal communication. Since a high laryngeal position would restrict vowel range but only minimally restrict consonantal features of speech, this asynchronous timetable would argue for a much more protracted evolution of auditory ad articulatory mechanisms for the latter. He suggests that this is reflected in the far more categorical, modular, and semantically important nature of consonantal features in modern language.

What kind of evolutionary dynamic was responsible for this human change?

Focusing on the effect that the medium of communication has on human cognitive evolution is MD's most important contribution. In particular, he demonstrates that mental evolution can be characterized by an increasing reliance on external cognitive prostheses (mostly in the form of communication aids) and that this greatly amplifies cognitive capacity beyond the limits of the brain. Unfortunately, his account of evolution is framed as a sort of hindsight narrative, whose motivation is clearly to justify a componential theory of modern cognition for which he would like to find corresponding origins stories. This leads him to postulate a series of discrete stages, each characterized by major revolution in communication media.

His most original and idiosyncratic contribution is the suggestion that there was a discrete mimetic stage in human evolution that preceded the appearance of language. MD supposes that an unprecedented and now superseded form of human communication and culture flourished during the time of Homo erectus. This caricature of a punctuated equilibrium theory seems entirely procrustean to me. Though the scenario offers a narrative bridge between a time when our ancestors were as poor at learned vocalizations as chimpanzees and our present vocal prolixity, there is little more justification. In any case, this imaginative scheme is unnecessary, even to support most of the general claims he wants to make about human cognition. Modern human mimetic predispositions almost certainly diverged from our more ape-like ancestors during this period, but side by side with increasing language abilities, as an important supportive adaptation for learning language, and part of an integrated suite of adaptations, not some independent stand-alone cognitive-communicative trick.

Along with many archeologists, MD and PL assume that the long-term evolutionary stability of erectus' tool-aided hunting adaptations indicates mental stagnation, and that the sudden flowering of tool diversity and durable representations in the late paleolithic is a marker for the sudden development of language. Both suggest that speech superiority was the cause of the elimination of erectus and archaic sapiens populations and the expansion of anatomically modern sapiens.

I consider it a kind of hubris to imagine that the rapid changes in technology and habitat that began at the end of the old stone age and led to the accelerating rate of social change and habitat destruction that has followed in the ten and a half thousand years since, is somehow a more successful evolutionary strategy than one that was stable throughout the Old World for a million and a half years. There was clearly a transition in technology in the upper paleolithic, but it is not clear that this reflects "advance" or a forced change in foraging brought on by an unsustainable or even self-undermining living strategy.

The differences in the way that TD incorporates brain size, intelligence, and evidence for vocal control into the problem of language evolution, as compared with both PL and MD, provides a very different view of the brain-language link. Whereas PL and MD effectively treat language as added on top of an otherwise slightly more intelligent ape brain--an adaptation in the medium of expression of mental processes--TD treats it as a core source of selection pressures driving brain and vocal evolution. If language were a relatively late adaptation then it would have relatively little to teach us about the structure of the human brain and predispositions of the human mind. Adaptations for language learning and use would be secondary to other more primary hominid adaptations such as general intelligence or, as MD suggests, some earlier nonlinguistic form of communication. In contrast to this view, both TD and SP argue for a protracted co-evolution of brain and language, in which early language use by our ancestors played a significant role in changing both the structure of the brain and the unique features of the human mind. Though TD and SP differ markedly in their assessment of exactly how this modified the brain and mind, both consider the human brain to be specially adapted to language as opposed to pre-adapted for language by some more general cognitive mechanisms augmented by vocal adaptations.

In all scenarios but Symbolic Species, the hypothesized transition from non-language to language is explained in terms of more efficient communication, complexification, and progress. TD, however, argues that efficiency and complexity arguments are inapplicable to the initial transition. he argues that the first language-like communication evolved despite inferior efficiency and complexity (compared to existing non-language communication) because it offered the only means to public represent abstract future and counterfactual relationships necessary to establish and enforce social-sexual contracts.

What happened to the brain to make language possible?

In all but The Symbolic Species the brain is essentially treated as a black box in which either anything is possible or the structural details don't matter.

The advocates of IMLF theories assert that something must have changed in the evolution of human brains that embodies the knowledge of grammar etc that they postulate, but are essentially moot on any details. In a theoretical move that makes these theories almost non-falsifiable it is claimed that this anatomical substrate need not be localized or even an obvious part of the neural hardwiring. Calling it an evolutionary algorithm allows SP to take the escape route of invoking a hardware/software distinction.

The continuity theorists also tend to ignore the brain in all but a superficial way. The most that is claimed is the classic argument about selection for increasing intelligence and global brain size in evolution. PL, however, makes some additional predictions about localizing grammatical processing to motor systems, based on his recent investigations of agrammatic disturbances in Parkinsonism. It is not indicated how this system changed or how this shift in function was selected for.

The Symbolic Species takes the problem of describing human brain evolution as its central task. It rejects both increased encephalization-intelligence theories and modular addition of new language organs. It offers an analysis of the relevant genetics, a reassessment of the role of developmental mechanisms, and a theory of the relationship between global quantitative changes in brains and the effects on both wiring and function. This makes this approach the first "inside-out" or "bottom-up" approach. It also makes many of the claims empirically testable, which is a first for language origins theories.

In summary:

SP argues that the evolution of UG is the core innovation in language, and that it is the basis for a radical discontinuity in cognitive processes sepatating humans from other animals. He offers a more biologically sensible rephrasing of this IMLF hypothesis than his predecessors, by invoking natural selection as its cause. This is a significantly more plausible assumption than the evolutionary agnosticism of Chomsky or the simplistic hopeful monster approach of Bickerton. Unfortunately, he fails to actually offer any evolutionary or neurological account to go with this appeal to Darwinian processes. His strong modularity and innateness claims are supported entirely by the negative argument: It can't be explained in any other way. And so the bulk of the book, like others arguing the IMLF view, is almost entirely a compendium of linguistic examples purporting to show how inevitable this approach must be.

PL offers a diametrically opposed view in which innateness and modularity are flatly denied and a strong continuity between animal and human communication and cognition is assumed. His view has developed from an earlier focus on the role played by the descent of the vocal tract (found in his books of the late '70s and early 80s), to a more general motor-centered view. He hypothesizes a late appearance of speech, mostly confined to Homo sapiens, and argues that general learning mechanisms structured via action systems sufficiently account for apparent universals of grammar. This most recent book seems a little more strident and polemical than his earlier books in it's repudiation of the IMLF view and fails to critically analyze many underlying cognitive issues, such as language reference and comparisons to animal communication.

MD offers an account from the perspective of cognitive psychology and also the most detailed just so story. He does an excellent job of reviewing the links between cognitive issues and evolutionary issues, but is perhaps a bit too creative and procrustean in his three phase scenario. He falls into a trap typical of accounts that try to tell a story linking pre-human and modern mind and culture. He invents an intermediate stage (mimetic culture of Homo erectus) out of whole cloth to fill in a narrative gap in order to fit his theory of modern minds. He is however the only one to offer anything interesting to say about the role that external symbolic systems may play in amplifying neural cognitive abilities.

TD argues for a functional and cognitive discontinuity of language and yet denies the IMLF theory and specifically the UG assumption. He also takes a critical stand against theories of the evolution of increased general intelligence and suggests that symbolic communication and even speech have had a roughly 2 million year prehistory of constant evolution. This puts him at odds with major tenets of each of the preceding books. This is because he locates the discontinuity between animal communication systems and human language, neither in its structural complexity of its rule systems nor in its vocal medium, but in the difficulty of interpreting symbolic relationships. He also inverts the implicit evolutionary causality of most previous accounts by suggesting how some of the most robust changes in human brain structure in evolution have been the direct consequences of the their co-evolution with language. Perhaps the greatest weakness is that it only hints at an alternative theory of language learning, leaving a large promissory note for a future book to fill.

DEACON, TABLE 1

Author; Book title	Scenario or theory	appearance of first language	appearance of speech	role of general intelligence	innateness & modularity	critical physical adaptation	importance of coevolution	implications for cognition	symbol learning problem	syntax learning problem	position on functionalism	hominid fossil scenario
Terrence Deacon; Symbolic Species	biological & semiotic theory with evolutionary scenario	Homo habilis first to use symbolic communication	early Homo erectus develops vocal-motor control	no brain size-intelligence-language link but learning biases	innate learning biases no innately modular language faculty	homeotic gene change -> prefrontal disproportion	symbolic communication as the source of brain evolution	reorganizes learning and enables a new mode of cognition	primary evolutionary problem	syntax constantly coevolves with human learning biases	languages adapt to learners/users predispositions & constraints	habilis; tool-aided foraging -> symbols mediate coop. pairbonding
Steven Pinker; Language Instinct	theory with no scenario	?	?	language module independent of intelligence	innate modular universal grammar and mentalese	uniquely human "language faculty" evolves in brain	important factor for genetic assimilation of grammar	cognition is modular language faculty is separate	ignored by assuming innate mentalese vocabulary	innate algorithm template for all possible language syntax	most language structure non-functional w parity selection	unspecified period of language use under selection
Philip Lieberman; Eve Spoke	both theory and scenario	incipient beginnings in archaic Homo sapiens	anatomically modern Homo sapiens	increasing intelligence precedes language	no distinct "language faculty" only general learning is necessary	motor specialization for tools -> brain size increases -> larynx descent	no role	cognition same as in primates just aided by language	?	not modular parasitic on the motor planning system	syntactic function borrowed from action structure	erectus' gestural "language" -> sapiens spoken language
Merlin Donald; Origins of the Modern Mind	more scenario than theory	language-like gestural communication in Homo erectus	anatomically modern Homo sapiens	general intelligence -> mimetic intelligence -> language	general -> "mimetic" intelligence -> speech adaptations	brain size increase, larynx descent, motor speech specialization	source of mimetic & general intelligence	mimetic predispositions underlying aesthetics	considered only in terms of medium effects on cognition	intermediate position, intelligence & innate templates	unclear but appears to assume functionalism	3 distinct stages; ape-australopith, mimetic erectus, modern articulate
Derek Bickerton; Language & Species	theory with no scenario	grammarless protolanguage in erectus (?) true Lang. in sapiens	anatomically modern Homo sapiens	evolution of intelligence uncoupled from language evolution	innate modular universal grammar and mentalese	uniquely human "language faculty" added to brain	no role	cognition is modular language faculty is separate	?	innate algorithm template for all possible language syntax	radical non-functionalist theory of syntax	Homo erectus (?) using grammarless protolanguage

MOTHER TONGUE

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Book Review Articles

Campbell, Lyle (1997) *American Indian Languages: The Historical Linguistics of Native America.* Oxford Studies in Anthropological Linguistics. New York/Oxford: Oxford University Press. Pp. i-xi, 1-512; with 27 maps. $75.00.

Reviewed by
Ken Hale
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This is an excellent book, an extraordinarily useful volume for anyone whose work and interests involve languages of the Americas or, more generally, the methods and results of historical linguistics. The book contains a range of information which answers, or is relevant to, virtually all of the comparative and historical questions one could expect to answer on the basis of the extensive published record on the indigenous languages of the Americas, a record well represented in the 53 page bibliography. Furthermore, it includes much information not readily available heretofore, together with a lot of valuable discussion by the author, one of the most knowledgeable Americanist linguists of this era. In short, this is a true and thoroughly authoritative handbook.

The first chapter is entitled "introduction", but it is not like any introduction I have ever seen before. It covers a multitude of important topics not included in the body of the book, ranging from the issue of language names to fake languages, and the issue of language endangerment. Appended to this chapter is a discussion of Native American pidgins, jargons, and trade languages, including a listing of them which is exhaustive, so far as I can tell.

The second chapter is a complete history of Native American historical linguistics, from the earliest comparative work in the seventeenth and eighteenth centuries to the more familiar Americanist comparative work of the late nineteenth and early twentieth centuries. Campbell makes the interesting observation that insightful comparative work by people like Jonathan Edwards Jr. (Algonquian) and Filippo Salvatore Gilij (Cariban, Maipurean) predated and was in some ways superior to that of their coeval, Sir William Jones. Moreover, these early Americanists had a clear conception of the notion "related language" and of the evidence needed to establish what we now speak of as "genetic" relationship. Gilij understood well the problems inherent in loan words, nursery words, accidental similarities, and areal diffusion. And the seventeenth century students of Algonquian, Roger Williams and John Eliot, already knew about sound correspondences, as did Gilij in the following century for Maipurean and eight other lenguas matrices 'mother languages' (language families) which he recognized in the Orinoco area of Venezuela. This chapter also includes an appendix comparing the major classifications of North American languages.

Chapter 3 surveys the major ideas concerning the origin of Native American languages. It is a good essay to read for one who harbors any doubt about the fact that there are highly problematic aspects to the use of nonlinguistic (e.g., dental and mitochondrial DNA) data in support of the ambitious and daring tripartite linguistic classification developed in Joseph Greenberg's Language in the Americas. Campbell repeats, and cites convincing published discussion in support of, the caution that "we simply cannot expect, let alone assume, a priori, that linguistic history correlates well with human biological history (p. 100)."

Chapters 4 through 6 present, to the extent that it is possible to do so, the complete classifications of the indigenous languages of North, Middle, and South America, with correspondingly full reference to the relevant published literature. The location of every language is given, and an indication of the size of the present speaker population of each language in the endangered category is also given—special tags are supplied for languages which are obsolescent (have fewer than 100 speakers), moribund (fewer than ten speakers), or extinct. Maps are assembled at the end of the text, before the endnotes. In these chapters languages are grouped together in families only if the relationship is generally uncontested. Thus, in this classification, North America has 57 families, Middle America has 17, and South America has 119. More distant relationships and higher level groupings are discussed in two subsequent chapters—these latter will be of special interest to the readers of Mother Tongue.

Chapter 7 deals with the methods used in establishing distant genetic relationships. It amounts to a "text book" on the methods and pitfalls which it is wise to keep in mind in doing comparative work aimed at establishing genetic connections, both near and distant. The pitfalls (e.g., nongenetic resembances due to chance, borrowing, onomatopeia, sound symbolism, nursery forms, typology, multilingualism and consequent grammatical merger, misanalysis of forms and other mistakes, and general insufficiency and misinterpretation of data) are generally well understood, though not always avoided, by serious students of linguistic relationships. They are treated fully in this chapter, and they are exemplified by means of relevant case studies. A general conclusion which must be drawn, in my opinion, is that these factors in aggregate, and in combination with ordinary linguistic change, result in the circumstance that genetic relationship, at some point, will not be provable. And the point at which this effect is fully in force, may be quite early in the years subsequent to language differentiation. I would join those who say that a separation of 10,000 years or more is too great to permit proof of genetic relationship. It should perhaps be pointed out that this number, or any one like it, is in reality a rhetorical device—we don't really know what number of years to assign to the time of separation which is too distant to permit us to determine that two sets of languages are related. I personally believe there is such a time and that it is some time greater, but not too much greater, than that which represents the maximum separation within Indo-European, Uto-Aztecan, Pama-Nyungan, and other families with which I am familiar. The figure 10,000 is just a guess. On the other hand, I remain committed to the notion that "long distance relationships" can be studied, I simply put these between 5,000 and, say, the rhetoric 10,000 years, rather than more distant. The reason I believe that one can work within the hypothetical 5,000-10,000 year range is due to something I learned from Joseph Greenberg's 1957 Essays in Linguistics, a book which I referred to regularly as a graduate student. What I learned there was that one of the best kinds of evidence for genetic relationship consists in what Antoine Meillet called "shared aberrancy"—Greenberg's example was English good/better/best beside German gut/besser/best. This impressed me enormously, and I made use of it to argue that ablaut pairs documented in J. P. Harrington's evidence for the Kiowa-Tanoan genetic relationship made his proposal virtually incontrovertable. To quote Greenberg's characterization of the comparable Germanic case, "the likelihood that all this is the result of chance is truly infinitesimal." Campbell quite correctly points out that uncritical acceptance of this kind of evidence must be tempered somewhat, citing relevant examples; it is nonetheless strong evidence and, like regular sound correspondence, it is to be included in the be best sort of evidence. Moreover, it is at least possible for "shared aberrancies" to linger long and to be used, with due caution, as a strong argument for distant genetic relationships—for example, Australian irregular monosyllabic verbs are among the strong evidence for genetic relationships, presumably distant, between Pama-Nyungan and Non-Pama-Nyungan languages.

Shared aberrancy can be abused, of course, just as apparent and spurious sound correspondences can. I sometimes use the following example to demonstrate the possibility of abuse. The Tanoan language Jemez shares with Algonquian languages three interesting accidental "quirks":

1. In the Jemez verb word, the prefixal position following person agreement and before the verb stem is occupied by an element which marks a heterogeneous variety of categories, including voice, noun class, and number. It is realized as an "effect" on certain stem-initial consonants and as /l-/ before a vowel or /h/. The other Kiowa-Tanoan languages have a corresponding element, with various functions, realized as an apical consonant, nasal or stop—thus, the range of realizations in the family is /l ~ n ~ t/. In Algonquian, the apical stop /t/ is inserted between (subject or possessor) prefixal agreement and a verb or noun theme beginning in a vowel.

2. Jemez, in conformity with the principle of so-called "inverse number marking" in Kiowa-Tanoan, exhibits the following pattern of number marking:

	Singular	Plural	
Animate	veela	veelae-sh	'man'
Inanimate	tyetubae-sh	tyetuba	'box'

In Algonquian, the ending *-ali is reconstructed by Bloomfield for the obviative singular of animate nouns and the plural of inanimate nouns.

3. As is well known, the bilabial nasal [m] often appears in second person pronouns and in second person agreement morphology in languages widely distributed in the world. This observation is involved in much speculation about language relationships, and the issue is taken up in

detail by Campbell in Chapter 7. In this light, it is not surprising then that a bilabial, nasal or stop (depending on the nasality of the following vowel), appears in nonsingular second person agreement prefixes in Jemez (bilabials also appear in this function in other Kiowa-Tanoan). What is less expected, is that the velar stop /k/ also appears as a second person marker in Jemez—it appears there in singular reflexive, indefinite object, and benefactive prefixal morphology. A velar stop appears in second person agreement morphology in other Kiowa-Tanoan languages as well. In Algonquian, *ke- is reconstructed for the second person prefix attested generally in Algonquian in both nominal and verbal morphology.

The ways in which all of this could be used and abused is probably not lost on anyone.

For the first example, one might argue that the present day Algonquian phonological principle according to which /t/ is inserted between a prefix and following vowel is the residue of a reanalysis of an "old" voice or class marker, still found as such in Kiowa-Tanoan. The fact that an apical consonant is involved in both language families is encouraging, and one need not be upset by the variety of consonants involved (t ~ n ~ l), since we have plenty of evidence of such correspondences elsewhere, i.e., in other language families. But this is abuse, I think, since we have no other evidence for these apical correspondences between Kiowa-Tanoan and Algonquian. Nor do we know how this element should be constructed within Kiowa-Tanoan (the best ideas we have come from the derivations found in Laurel Watkins and Parker McKenzie's 1984 A Grammar of Kiowa, Nebraska, and these are internal to Kiowa itself). We really cannot say, therefore, that this is not pure accident, comparable to the corresponding accident represented by the formally rather similar pre-stem d-element of the Athabaskan voice system. Karl Teeter's dramatic use of epenthetic [t] to support Sapir's Algonquian-Ritwan unity (now called Algic) leaps to mind here, of course, since that helped to legitimate a controversial proposal. But the Algic case is supported by other evidence, and the principles governing epenthesis in both Wiyot and Algonquian seem to be phonological, at least in the sense that the phenomenon can be defined in strictly phonological terms (an initial /V/ in Algonquian, initial /hV/ in Wiyot). In Kiowa-Tanoan, by contrast, while phonological considerations do play a role in determining the surface realizations of the element at issue in this comparison, the phenomenon itself has to do with aspects of the morphosyntax of the languages. And in this connection, there is an aspect of all of this which is probably not accidental. Let us imagine a pre-Algic time when the epenthetic /t/ was in fact a prefix /t-/ with some function or other, like Athabaskan /d-/, and like Kiowa-Tanoan /t- ~ n- ~ l-/, perhaps reconstructable there as something like /d-/ (as in the underlying Kiowa form assigned to this element by Watkins and McKenzie). This gives us a "morpological profile" of the form Prefix+D+Verb (D some apical) shared by Algonquian, Kiowa-Tanoan, and Athabaskan. Recall that we cannot establish a sound correspondence for D. But suppose we say that the languages are simply so distantly related (Athabaskan is not even in the putative Amerind group) that we can no longer establish sound correspondences. But this does not prove that they are not related—there can be no such proof, of course. Can we use this morphological coincidence to argue for the relationship?

Could this coincidence possibly be entirely accidental? I think the answer has to be the following. We cannot use it, but it may well not be accidental. And this has to do with the dangers of using another method discussed by Campbell in Chapter 7, namely "positional analysis", used so effectively by Dell Hymes in his study of Athabaskan and Na-Dene. The idea is that the relative linear ordering, within a word, of bound functionally equivalent elements is relatively stable through time and should, therefore, provide evidence of genetic relationship. There is a serious problem with this, however. The very "stability" of morpheme ordering is often due not to genetic relationship but rather to general principles of grammar which place certain elements closer to others in the hierarchical organization of clauses. In "synthetic" constructions, derived by what is now called "Head Movement"—e.g., raising and conflation of a syntactic nucleus, say a verb, with a higher nucleus, say a causative formative, or a tense formative with attached agreement morphology. The consequent ordering of affixes follows from the general principle called the "Head Movement Constraint", which prevents "skipping" an intervening head. Thus the coincidence at issue here is most probably "genetically accidental" but, depending on the details, "non-accidental" from the point of view of universal grammar. If this is right, then, in the absence of other evidence, the relative ordering of bound morphology is of little use in establishing a genetic relation between Kiowa-Tanoan and Algonquian, or any comparable but otherwise unsupported relation.

The second case relies on the "quirky" circumstance that the nominal systems of Algonquian and Jemez share the property that one and the same suffix is used to mark singular number with one class of nouns and plural with another. Of course, the assignment is reversed in the two families—in Jemez the marked form is singular with inanimates, plural with animates; the reverse is true in Algonquian. It might be argued that this is a result of some sort of "historical confusion", inasmuch as Jemez shows overlap in the dual—both noun classes using the marked form. But here again, this interesting coincidence, is of little use. First, the two systems are not comparable—in Algonquian, the morphology is involved not only with number but also obviation; and in Kiowa-Tanoan, it is involved in a complex system of noun classes diversely realized in morphology. In neither case is it possible to reconstruct systems which are comparable. The more carefully one studies these systems within the two families, the less similar they appear to be across the two families. Second, there is no hope whatsoever that the Jemez and Algonquian forms can be shown to be cognate. It's not that *-ali couldn't be related to a form involving a lamino-alveopalatal fricative—it could, in theory; in fact the plural inanimate suffix is -ash (< *-ali) in Massachusett (irrelevantly, of course). It's that no sound correspondences at all can be established between Algonquian and Jemez. Moreover, it is virtually guaranteed to be the case that the reconstruction of the Kiowa-Tanoan system of suffixal number marking will not include anything resembling Jemez -sh (Jemez /sh/ does not continue a fricative, but an aspirated apical stop /t^h/ or affricate /c^h/), and given the difficulty of reconstructing the nominal number and class marking system internal to the Kiowa-Tanoan family itself, it is certainly not possible to say that a "clean" inverse marking system will ultimately emerge, let alone that it will involve endings looking anything like the Algonquian suffixes cited.

I have always had a special fondness for cases of the third type, i.e., the situation in which languages coincide in showing an unusual realization of a particular grammatical category. The argument would go like this in the case at hand. It is unusual for the second person to be realized by a prefix of the form [k(V)-]. It is therefore significant if two different languages, or language families, share this property. The usefulness of this type of evidence depends on the reliability of the basic assumption, namely, that the forms in question will prove to be cognate. Unfortunately, we cannot establish this, and it would be perverse simply to assume that the coincidence itself is great enough, and a priori unlikely enough, to "establish" the relationship on the grounds that the coincidence is beyond the realm of chance. Pushing the issue, we know that the Algonquian prefix *ke- appears wherever the subject or possessor includes reference to the second person, including the category normally thought of as a first person, i.e., first person inclusive. In Taos, a Tanoan language, the prefix k- appears not only in second person singular forms (as in Jemez) but in the first person nonsingular as well. Though the Taos prefix system does not distinguish inclusive and exclusive first person, the distribution of Taos k- invites speculation that its appearance in the first person nonsingular is due to a historical first nonsingular inclusive use, possibly cooccurring with a first nonsingular exclusive set, lacking the k- prefix, corresponding to the first nonsingular of present day Jemez. These considerations make Algonquian and Kiowa-Tanoan seem still more alike in this regard. However, the full picture is more complicated—in Tewa, the velar stop prefix (voiced g- in that language), is exclusively first nonsingular; and in Kiowa, the similarly voiced prefix g- is found in paradigms in association with first, second, and third persons. There is no way to tell, at this point, whether the eventual reconstruction of this prefix (a project which must take into consideration the full and notoriously complex Kiowa-Tanoan prefix system) will result in a form which resembles the Algonquian prefix *ke-. If it does, we are left with the basic question—is this an accident? Without additional evidence, it seems to me that we must consider it an accident. These languages are not alone in having a velar in association with the second person. Two examples come readily to mind—Armenian (second plural or formal) -k; Maori, Niuean (etc.) koe. These can be shown to be entirely beside the point, but only because we know a lot about the history of these languages. We do not have the information we need to determine whether the Algonquian/Kiowa-Tanoan comparison is beside the point, or not beside the point. In this situation, I think personally that we should come down on the conservative side of the question.

An especially important point made in Chapter 7 is the following. Established and generally accepted language families which involve some time depth typically exhibit formal correspondences which are not obvious on inspection and cannot be "picked up" either by simply comparing forms which look similar or conform to some a priori notion of what a plausible sound correspondence might be. Consider, for example, the Northern Paman languages spoken north of the Watson River on Cape York Peninsula. These are clearly related to the Middle Paman languages south of the Watson. Correspondences are not immediately obvious, however, as the following comparisons demonstrate (ng = velar nasal; th, nh = lamino-dental stop, nasal; j, ny = lamino-alveopalatal stop, nasal; tr, dr = apical stop with rhotic release; ′ = glottal stop; V: = long vowel):

Linngithigh	Wik Munkan	Paman	
tru	nhint	*nyuntu	'thou'
a'a	ma'	*mara	'hand'
nggo	pungk	*pungku	'knee'
ina-	nhi:n	*nyi:na	'to sit'
ndra-	want	*wanta	'to put'
tha-	path	*paja	'to bite'
ma	pam	*pama	'person'
nga	pung	*punga	'sun'
ayo-	ngay	*ngayu	'I'
adhi-	me:j	*ma:ji	'hungry'

On the basis of this specially selected short list it is possible to characterize what is going on. Northern Paman languages are "initial dropping languages", and the Wik languages of Middle Paman are "final reduction languages", reducing all final vowels to zero (or schwa, depending on context). Northern Paman stops are realized as voiced following nasals or long vowels (*pungku > nggo, *wanta > ndra-, *ma:ji > adhi-). The apical stop is released in a rhotic (brief apical trill, as in *nyuntu > tru). Where the form began in a nasal, all nasality was dropped from the initial syllable under initial dropping (as in the preceding example). Initial dropping eliminated both the initial consonant and following vowel, if short, except before a glide or glottal stop (hence retention in *mara > a'a, *ngayu > ayo-). The effect of an initial consonant may be felt in the residue (as in *nyuntu > tru, where the initial laminal prevented the usual lowering of *u to /o/). A long vowel in the initial syllable reduced to short (*nyi:na > ina-, *ma:ji > adhi-).

While a short list can now be used to illustrate the major changes, it took a lot of study to arrive at the point where such an illustrative list could be conceived of, let alone assembled. In fact, it took some time to determine that the languages were relatively closely related, and then to set about determining the historical developments responsible for the synchronically observable state of affairs. The point here is that some work has to be done to establish correspondences, even where the relationship is generally accepted or intuitively obvious. We know this from comparative work in Indo-European, Semitic, Finno-Ugric, Bantu, Uto-Aztecan, Algonquian, etc., as well as in less well known and initially "exotic" cases like the Paman languages, which, post hoc, can only be regarded as rather close in relationship. I take this to be problematic for multilateral or mass comparison, which by contrast tends, for very distant and questionable relationships, to come up with comparisons which are more direct and transparent looking than those which typically seem to be found in close relationships—this is a sort of "paradox of similarity". The reason for this, I suppose, is that comparisons in very distant and hypothetical relationships are in general too sparse to permit determination of what the actual correspondences are. In the Paman case, for example, a correspondence between initial dropping and initial retention only becomes obvious or credible when a large number of comparisons (in fact all defensible comparisons) actually show it.

It should be pointed out, however, that there is an aspect of multilateral comparison which is absolutely true. Just as the validity of Indo-European would have been difficult, perhaps impossible, to establish on the basis of, say, Hindi and Modern Irish alone, so also the establishment of Paman probably could not be established on the basis of Linngithigh and Wik Munkan alone—this would lead to serious mistakes in our understanding of the comparative picture (cf. Campbell's discussion of the "binary comparison red herring"). Instead, in actual fact, some thirty Paman languages, and a large number of Pama-Nyungan languages outside Cape York Peninsula, played a fundamental role in helping to define the relation between some eight somewhat scantily documented Northern Paman languages and their relatives to the South. Still, relationships are rather close here (probably well within the 5,000 year range) and correspondences are multitudinous. It is in the circumstance of relatively close relationships that the multilateral procedure is most useful and, in fact, almost indispensible, in refining the details of sound correspondence and morphosyntactic comparison. The problems inherent in applying it in questionable cases, and the benefits of applying it in promising cases, are fully discussed and illustrated by Campbell in Chapter 7.

Chapter 8 deals with another aspect of distant genetic relationships, namely, the proposals themselves. After listing, and dismissing, a number of far-fetched proposal, Campbell considers a number of proposals which he takes seriously, assigning to each a probability rating, and another rating corresponding to his own confidence in the first. Three proposals are treated in some detail, and offered as case studies—Macro-Siouan (Siouan-Iroquian-Caddoan-Yuchi), Aztec-Tanoan (Uto-Aztecan and Kiowa-Tanoan), and Quechumaran (Quechuan and Aymaran). Dozens of other proposals are also treated, in varying detail, with the result that most, if not all, serious hypotheses of linguistic relationships among Native American languages are covered in appropriate detail in the book. Although Campbell's evaluations might sometimes be seen as rather stern, they are always well reasoned, and where he feels it is reasonable to develop the arguments behind a critical appraisal of a given proposal—e.g., Coahuiltecan, Penutian—he does so as carefully and as fully as possible, consistently making reference to the methodological principles and caveats explicated in Chapter 7. For the long-range proposal which is best known to me, Aztec-Tanoan (0% probability, 50% confidence), Campbell carefully demonstrates the reasons for his assessment, systematically using the principles of Chapter 7 to reduce the Whorf-Trager putative cognates to a mere five to seven comparisons which survive the tests. This accords pretty well with my own assessment based on quite a lot of work in comparative Uto-Aztecan and comparative Kiowa-Tanoan.

Chapter 9 completes Campbell's fine coverage of the historical linguistics of Native America with a thorough survey of the linguistic areas of America, keyed to the maps which follow this final chapter of the book. A linguistic area is a region in which structural features are shared among languages without respect of language or family boundaries. And areal linguistics, which concerns itself with this phenomenon, is crucial to historical linguistics insofar as that is concerned with determining the true genetic heritage of languages and language families. Campbell is nearly as demanding of proposals of linguistic areas as he is of proposals of genetic relationship. And this is of course the correct attitude, since to put something down as an areal effect can be as right or as wrong as can the attribution of a shared feature to common ancestry. In addition to three well established linguistic areas—the Northwest Coast and the Southeast in North America, and the large Mesoamerican linguistic area—this chapter discusses some ten other proposed North American linguistic areas and eight South American linguistic areas.

I would like to use the areal phenomenon to illustrate why it is that I am so positive about this book and its generally cautious and conservative tenor. Within the Colombian-Central American area, much of which has been very carefully delineated and supported in the work of the Chibchanists Adolfo Constenla Umaña and Denis Holt, their exists a small family, called Misumalpan, consisting of three extant languages in the Caribbean regions of Honduras and Nicaragua—namely, Mayangna-Tawahka (Northern Sumu), Ulwa (Southern Sumu), and Miskitu—and a pair of closely related languages, now extinct, which go by the names Matagalpa (Nicaragua) and Cacaopera (El Salvador), Matagalpan when referred to together. The family-level name "Misumalpan" is acronymic, based on the language names Miskitu, Sumu, and Matagalpa.

Misumalpan has sometimes been linked to Chibchan, but as Campbell notes in Chapter 8, the evidence for this is weak. On the other hand, Misumalpan itself has never been challenged, so far as I know. It is nevertheless this presumably much more credible linguistic entity that I want to use in trying to justify my general reluctance in proposing, or accepting, linguistic relationships. I will say at the outset that I believe in Misumalpan, but the evidence for it has to be sought elsewhere, I believe, than in readily accessible lexical material. In fact, standard lexical comparisons are of little use, if any use at all. Consider the following:

Northern Sumu	Southern Sumu	Miskitu	
yamni	yamka	yamni	'good'
sangni	sangka	sangni	'green'
bulni	bulka	bulni	'spotted'
pihni	pihka	pihni	'white'
pauni	pauka	pauni	'red'
lalahni	lalahka	lalahni	'yellow'
pamni	pamka	pamni	'narrow'
ingni	ingka	ingni	'bright'

From this alone, it would appear that Miskitu is closer to Northern Sumu that the latter is to Southern Sumu. But while the Sumu comparisons can be considered truly representative of Sumu linguistic heritage, the Miskitu comparison is spurious. In Sumu, most adjectives consist of a categorially neutral root in combination with an ending (-ni in Northern Sumu, -ka in Southern Sumu) which functions elswhere in the language as the marker for the so-called construct state and third person possessor agreement, appearing on the possessum. The adjectival and nominal uses of these endings conform to a single morphosyntactic principle in the structure of nominal constructions. The suffix -ni appearing on the Miskitu adjectives above has no other function in that language—it is an entirely alien element there. While Miskitu has construct state morphology, it is not realized by -ni but rather by a slightly more complex set of endings incorporating a familiar classificatory dimension, -a (inalienable), -ka (alienable$_1$), -ya (alienable$_2$). The only reasonable conclusion, it seems to me, is that Miskitu has borrowed these adjectives from Northern Sumu. If one continues to list adjectives from the three languages, one sees eventually that Northern and Southern Sumu continue to share the radical elements, while Miskitu departs from Sumu and presents adjectives (e.g., sirpi 'little, tara 'big', yari 'long', kunghku 'short', etc.) bereft of any construct morphology and found only in Miskitu.

The observation that Miskitu borrowed these items from Northern Sumu, rather than Southern Sumu, is consistent with the following rather striking comparison:

Northern Sumu	Southern Sumu	Miskitu	
yang	yang	yang	'I'
man	man	man	'you'
witing/witin	alas	witin	'he/she/it'

It is reasonable to ask how I can possibly be anything other than entirely sanguine about the relationship of Miskitu to Sumu in light of this set of forms. But notice that, in relation to the third person, Miskitu agrees again with Northern Sumu in opposition to Southern Sumu. Specifically, it agrees with the Twahka dialect form witin. Third person pronouns are notoriously unstable in languages the world over, and it is virtually impossible that witin goes back to an ancestor shared by Miskitu and Twahka. This third person form appears to be an innovation in Northern Sumu itself, it seems to me, and most likely a borrowing from that language into Miskitu. We are left then with the first and second person pronouns, identical in the three languages. I am not very sure about these either. In short, the entire set of Miskitu independent pronouns could have been borrowed from Sumu. For one thing, only the Sumu languages offer evidence of any antiquity for these elements. They appear in affixal form in the personal inflections of verbal and nonverbal predicates and are, therefore, integrated into the inflectional system, a circumstance which takes time to develop. While the Northern Sumu first and second person endings can be clearly related to the independent pronouns, the corresponding inflections in Miskitu are evidently from a different set. Though the Miskitu set might, of course, be distantly related to the Sumu endings, they are not straighforwardly related to the independent pronouns—this is true especially of the first person:

Northern Sumu	Miskitu	
-yang	-na	'first person'
-man	-ma	'second person'
-Ø	-a	'third person'

Southern Sumu employs the same set as Northern Sumu on stative and non-verbal predicates, but it has somewhat more eroded forms of these endings in the verbal paradigms, suggesting some antiquity. The fact that *-yang is clearly evident in the Sumu inflectional system and completely absent in

Miskitu, where *-na is found instead, is consistent with the idea that yang, at least, is recent in the evolution of Miskitu. The Miskitu set of person inflections represent another, distinct, potential antiquity. They recall, incidentally, the famous widespread n/m pronominal pattern discussed in Chapter 7 (the "Pronoun Argument") and given special import in Native American comparative linguistics by Morris Swadesh, Joseph Greenberg, and Merritt Ruhlen, among others. In any event, it is the morphologically bound pronominal elements, rather than the independent pronouns, that are most likely to be representative of the true relationship between Miskitu and the Sumu languages. The relationship is genetically more distant than that initially suggested by the simple adjectival and pronomnal comparisons given above, in which compared items are identical—or nearly so. Exceptions to identity are minor—consider Northern Sumu parahni beside Miskitu prahni 'short' in which the latter language has undergone a vowel deletion process typical of it; and consider Southern Sumu sana-pauka 'dwarf deer (lit. red deer)' beside Miskitu snapuka, with similar vowel deletions; these forms are clearly borrowed (bearing, as they do, the tell-tale construct morphology), but they have undergone specifically Miskitu developments, making them trivially different from their Sumu sources.

For me, the lesson to be taken from this is the following: (1) inflectional morphology, indicates greater separation among the languages than do lexical items; (2) shared, generally identical, lexical material is almost invariably borrowed; (3) the lexicon is therefore of little use in determining the nature of the suggested genetic relationship (between Miskitu and Sumu, in this instance).

This final conclusion is not entirly warranted, of course, for one might look to nonidentical, or dissimilar, lexical comparisons for true cognates. In this connection, we might look at the pair Sumu nawah, Miskitu limi 'jaguar', suggested by Adolfo Constenla Umaña (1987, "Elementos de fonología comparada de las lenguas misumalpas", Filología y Lingüística 13:129-61). Pushing this idea, we naturally hope to find other instances of the sound correspondences suggested by this comparison—e.g., the pair Sumu sana, Miskitu sula 'deer'. Here, the correspondence n/l is repeated, and a new correspondence s/s is suggested as well. The failure of vowel correspondences in these comparisons is bothersome, of course, but it might be attributed to the construct morphology in some as yet undetermined way—the two Miskitu nouns belong to the alienable$_2$ class, taking the ending -ya, but also taking the infix -â-, giving lâmya and swâlya. This is not unpromising, in and of itself, I suppose, but the lexicon is in truth not particularly forthcoming in material that might establish a body of "true cognates" which might include these forms.

By contrast, inflectional morphology provides the evidence which convinces me of the reality of the Misumalpan family and, specifically, that Miskitu is related to Sumu and Matagalpan. The possessive paradigms, built upon the construct state, take the following form in Misumalpan (1, 2, 3 represent the person categories):

	Matagalpan	N. Sumu	S. Sumu	Miskitu		
1	-ki	-ki	-ki	-i	-k-i	-y-i
2	-ma	-ma	-ma	-a-m	-ka-m	-ya-m
3	-ka	-ni	-ka	-a	-ka	-ya

The plain construct and the third person possessive are identical. Northern Sumu is innovative, introducing the ending -ni for the construct. This is the result of a wholesale replacement of third person inflectional morphology by morphology which originally realized the combined category 'first inclusive' and 'impersonal', categories retained as -ni in Southern Sumu and Matagalpan nominal inflection. This shift is one of the principal differences between Northern and Southern Sumu. The element -ka still exists in Northern Sumu, where it remains as an impersonal in some nominal forms (compare di-ni-t 'voice (3, construct)' beside da-ka-t 'voice (impersonal)'). It is probable that the velar stop found in the first person ending should also be attributed to a basic construct formative -ka. In any event, this element is present in all of the languages, including Miskitu, where it is restricted to alienable$_1$, the largest nominal class in the language, and the class to which most items in the impressive body of English loans belong.

The Miskitu construct is more complex, in appearance, than that of Sumu, and consequently, the possessive paradigms are also more complex in appearance. This is due, of course, to the fact that the Miskitu paradigms reflect the noun classes recognized in the language. Because of the make-up of the Miskitu forms, we are led to believe that the first person morphology is "really" -i, not -ki, the latter being due to a reanalysis in Sumu and Matagalpan of a form consisting of the construct element -ka plus -i, with a familiar type of vowel deletion. I happen to believe that this is correct, in fact, and my faith in it is confirmed, I think, in the Matagalpan verbal inflections which support the idea of a set of person markers entirely lacking the velar stop and, thus, the idea that the first person ending is originally -i—compare the following set to the first of the three Miskitu sets:

	Matagalpan
1	-i
2	-am
3	-a

This suite of comparisons is almost enough to persuade me that Miskitu belongs with the Misumalpan languages. I am fully convinced once certain other facts of inflection are added to the picture, including, for example, the system of verbal endings defining the basic verb themes, transitive and intransitive (inchoative). In all Misumalpan languages, a suffix reconstructable as *-w(a) marks a class of intransitive verbs (roughly, verbs of the kind commonly labeled unaccusative or inchoative). The transitive counterparts of these in Miskitu are marked by means of the formative -k-, evidently not cognate with, but quite regularly correlated with, the Sumu formative -ta, as in the following comparisons:

Southern Sumu	Miskitu	
bah-wa-	krî-w-	'to break (intransitive)'
bah-ta-	krî-k-	'to break (transitive)'
lâ-wa-	lû-w-	'to pass, move (intransitive)'
lâ-ta-	lû-k-	'to pass, move (transitive)'

While only *-w(a) involves truly cognate morphology, so far as we know, the function of these elements belongs to the same grammatical system in both Sumu and Miskitu. And in the tradition of Hymes' "Positional Analysis", Misumalpan verb theme marking is by itself arguably compelling. But it is not really enough. What is enough, in my opinion, is a rather remarkable coincidence in the theme marking systems of Southern Sumu and Miskitu. In these languages certain transitive verbs and, interestingly, certain intransitive verbs of the semantic type commonly called "unergative" are specially marked by means of an ending whose consonantal component is a bilabial. The forms are -b- in Miskitu, -pa- in Southern Sumu:

Southern Sumu	Miskitu	
alh-pa-	klauh-b-	'to perforate'
balis-pa-	klû-b-	'to wrap around, cling to'
bit-pa-	pah-b-	'to sweep'
bui-pa-	sing-b-	'to winnow'
auh-pa-	ak-b-	'to bark'
bur-pa-	ring-b-	'to rumble (thunder)'
subuk-pa-	skut-b-	'to hiccup'

It is not possible, with any great certainty, to establish a p/b sound correspondence between Sumu and Miskitu, because lexical cognates are simply not forthcoming in great enough numbers, as we have seen. Nonetheless, it seems to me that the details of Misumalpan inflection—including the verb theme markers, with this virtually unique p/b correspondence—pretty well clinch the relationship.

Misumalpan today constitutes a linguistic area. Although Miskitu is almost certainly related to the Sumu languages, it is not for this reason that they are a linguistic area. The extensive sharing of linguistic material observed today is not due to genetic relationship but to a pattern of intensive bilingualism which began, evidently, in the 17th and early 18th centuries in the context of Miskitu military and commercial ascendancy, encouraged and abetted by the British. This development brought with it, among many other things, a Miskitu assault on Sumu communities resulting in the introduction into the Miskitu population of a significant Sumu component. The linguistic picture we observe today gives testimony which is strongly consistent with known historical events in the Mosquitia and suggests an 18th century period of intimate Sumu-Miskitu sociopolitical confrontation and integration in which some variety of Northern Sumu played a nontrivial role in the formation of Miskitu as we know it now.

What has happened since is this. Most if not all speakers of Sumu are also fluent speakers of Miskitu. This has been true for a long time, evidently, perhaps since the 18th century. Miskitu is considered the indigenous lingua franca on the Atlantic Coast, and until recently, Sumu communities have used that language for important community purposes (e.g., church services). In the Southern Sumu (Ulwa) town of Karawala, the population switched to Miskitu almost exclusively when Miskitu workers settled there in 1950, a painless switch since Miskitu as well as Ulwa was known by all inhabitants of the town. As a consequence of effectively balanced bilingualism, the Sumu and Miskitu languages exhibit the phenomenon which is sometimes called "grammatical merger", permitting us to say with only slight exaggeration that Sumu and Miskitu have the same syntax and virtually identical morphologies (in structure, though not in form); they have essentially the same idioms and, in short, it is possible to go from one language to the other by simply substituting the appropriate morphemes. Modern Misumalpan is a machine translator's dream. There are exceptions to this, but by and large, structural isomorphism is the modern state of affairs among the living Misumalpan languages. It is therefore a minefield for the comparativist. The languages are too similar. A layer of borrowings must be removed, and the effects of grammatical merger must be set aside before one can begin the task of determining what belongs properly to the common genetic heritage. It is in the nooks and crannies of inflectional morphology, I think, that ones must look in taking this task on.

As I mentioned at the outset, I am citing this Misumalpan case—and in particular, the problem of Miskitu in relation to that family—as an example of why I take the position of a "short ranger" and applaud the general point of view which comes through in Chapter 7 this book.

I repeat, I think this is a wonderful book. It is impossible for a book of this scope to lack mistakes, and I will cite two that I noticed, both in the nature of "typos". The Stockbridge Algonquian language which Jonathan Edwards Jr. spoke, Muhhekaneew, appears as Mohican (or Mahican) in the modern spelling, rather than Mohegan, the name of the Connecticut group associated with the Pequots. And on page 399 (fn. 103), the first vowel in the Towa term meaning 'at Jemez Pueblo [to the north]' should be the high back unrounded vowel, i.e, barred-i.

As a final comment about the book itself, I want to mention the footnotes, gathered together before the references. These are a "good read", even by themselves; they include indispensible information on language names, and many scholars' commentary which it would not have been convenient to include in the text but which is nonetheless of great interest and importance. The notes to Chapter 2 include biographical sketches of most of the principal figures in Native American historical linguistics, a joy to read.

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A Guide to the World's Languages. Vol.1: Classification, by Merritt Ruhlen. Stanford University Press, 1987. xxii + 433

Reviewed by Václav Blazhek, Přibram/Brno, Czech Republic

[Editor's Note: This review is reprinted from Asian and African Studies 1.1 (1992), pp.76-92, by permission from the editors, Viktor Krupa and Josef Genzor. Some minor corrections and other changes have been made.

I cannot overemphasize the importance of this review for Long Rangers, and for all the readers of Mother Tongue. The reviewer, himself a leading Long Ranger, has an encyclopedic command of historical linguistic literature affecting languages all over the world. This can be seen from the bibliography, itself a major work listing a large number of important books and articles. Blazhek's perspective as a Slavic speaker also allows him to cite many important works in Russian and other Slavic languages.

For another important review of Ruhlen's GUIDE, see that of ASLIP founder Harold C. Fleming, "Towards a Definitive Classification of the World's Languages", Diachronica 4 (1987), pp.159-223, reprinted in Mother Tongue, Newsletter 20 (1993), pp. 4-30. **JDB**]

The first version of this book was a self-published edition printed in 1975 [A Guide to the Languages of the World, Language Universals Project, Stanford University]. It contained information on roughly 700 languages. The revised edition includes about 5,000 languages, not in alphabetical order, but according to their genetic connections. Following volumes will be devoted to Language Data (vol.2) on about 2,000 better-known languages, and Language Universals (vol.3) on typology.

Part One, "Genetic Classification: Principles and Methods" (pp.4-23) presents a very important general introduction to the aims and methods of genetic classification of languages. Chapters 2-6 gradually cover all the world's language (macro-)families according to continents, always including a short history of the development of genetic classification of respective regions, and a list of basic literature. Chapter 2 is devoted to Europe (pp.24-75), with the Indo-Hittite, Uralic-Yukaghir and Caucasian families; Chapter 3 to Africa (pp.76-123), with the Afro-Asiatic, Niger-Kordofanian, Nilo-Saharan and Khoisan macro-families; Chapter 4 to Asia (pp.125-158), with the Altaic, Chukchi-Kamchatkan, Dravidian, Sino-Tibertan and Austric (Austroasiatic, Daic) families; Chapter 5 to Oceania (pp.159-190), with the Austric (Austronesian), Indo-Pacific and Australian (macro-families; and Chapter 6 to America, with Eskimo-Aleut, Na-Dene and Amerind (macro) families. Chapter 7, "Prospects for Future Research" (pp.252-274), concerns methodological errors in proofs of genetic relationships, low-level and large-scale groupings, and the origin and evolution of language. The last chapter, 8, "Genetic Classification of the World's Languages" (pp.275-378), contains the following tables: "Overview of language phyla," with the number of extant languages, the number of speakers, location, and representative examples of languages; "Index to the classifications," a list of 17 main (macro-)families; "Major language groups," with a well-arranged classification in (macro-)families and their branches and groups; and finally, "Complete classification" includes all 5000 of the world's languages and their detailed classifications. The book closes with indices of personal names, language groups, and individual languages. It is richly supplemented with tables.

The main source of information on the genetic classification of the world's languages were, besides Voegelin's (1978) well-known book, mostly important works from the early 1980s (e.g., Greenberg's Language in the Americas [1987]) and letters from many specialists in regional classification. The influence of the author's teacher, Professor Joseph H. Greenberg -- certainly the hardest-working linguist in world classification -- is perceptible. The memoranda, notes, supplements, and alternative schemes of classification which follow below, are gathered by the reviewer, mainly on the basis of sources probably unknown or inaccessible to the author.

[p.30] The discovery of a genetic language family, on the basis of comparative work, was probably first realized by Mahmud al-Kašghari, in the case of Turkic languages (his "divan" was written in 1072-74; Kondratov, 1974, p.38). The unity of other families had usually been recognized only on the basis of anecdotal information, such as Al-Idrisi's (D^{2}azirat-al-Qumr) report that the language of Javanese traders was intelligible to the inhabitants of Madagascar (cited by Kobishchanov, 1966, p.113), etc.

[pp.38-40: Indo-Hittite] Lexical resemblances among Greek, Latin, German and Czech languages were registered by the Czech humanist Zikmund Hrubý from Jelení (1497-1554) in his Lexicon Symphonum (1537, cited by Horálek, 1955, p.403).

[pp.57-60, 325-327: Indo-Hittite] The author ignores most of the extinct languages known only from epigraphy, ancient glosses, or from toponymy, e.g., Phrygian, Macedonian, Thracian, Dacian, Illyrian, Venetic, Messapic, Lepontic, Celtiberian, etc. although some basic sources for these languages are cited in the literature (p.61).

[p.59, 327] The classification of Celtic languages, based on genetic principles, is quite different (Schmidt, 1987, p.114):

1) INSULAR (GOIDELIC)
2) CONTINENTAL a) ARCHAIC + CELTIBERIAN (*KW = KW)
b) PROGRESSIVE (*KW > P): LEPONTIC, GAULISH, BRYTHONIC

[p.60] Old Prussian has been extinct since the 17th century, but there has been a very remarkable experiment to revive "New Prussian" in the town of Dieburg (near Darmstadt, Germany) since 1980 (Palmaitis, 1988) by the Tolkemita Society (descendants of immigrants from Eastern Prussia).

[p.65] The Hungarian-Finnish [= Finno-Ugric] relationship was recognized before Stiernhielm and Vogel (1671) by the Czech scholar and pedagogue Jan Amos Komenský (Comenius), in his work <u>Opera Didactica Omnia</u>, Amsterdam 1657 (Pra²ák, 1969).

[p.70: Yukaghir] "Chuvantsy" represents the Russian form of the ethnic name (in plural). The correct name of their language is Chuvan. The Uralic-Yukaghir genetic relationship is also discussed by authors other than Collinder and Harms, e.g., Tailleur (1959a, 1959b, 1962, 1965), Kreynovich (1958, 1982) and Nikolaeva (1988a, 1988b). On the other hand, specific Yukaghir-Altaic connections also exist (Sauvageot, 1969; Nikolaeva, 1986), and their character is not unambiguously areal. Possibly a more adequate explanation is to assume the independent position of the Yukaghir language family in Eastern Nostratic.

[p.74] The genetic unity of all Caucasian[1] [areal] languages is very questionable. The Kartvelian family belongs to the Nostratic phylum, while North Caucasic [Abxazo-Adygheian + Nax-Dage-stan) is a member of the Sino-Caucasic phylum. The connections of North Caucasic and Kartvelian are more likely areal, and their genetic interpretation is possible only at the level of a hypothetical common proto-language of both phyla (Starostin, 1989).

[p.75] The Basque-North Caucasic genetic relationship is also defended by Chirikba (1985), who works with North Caucasic reconstructions by Starostin and Nikolaev.[2]

[p.77] Eudoxos from Cizicus (2nd century BC), who noticed the similarity among the African languages along the coast of the Atlantic and Indian Oceans (according to Strabo, II,3,5, cited by Kobishchanov, 1966, p.113), was probably the first to recognize the affinity of the Bantu languages.

[pp.90-91] The exclusion of Beja from Cushitic is not generally accepted (Zaborski, 1981, 1985a, 1986). On the other hand, the results of a lexicostatistical analysis of common Afro-Asiatic lexicon confirm a closer connection of Chadic with the Berber-Libyan branches (Militariev, personal communication, 1987); the analysis of Afro-Asiatic numerals (Blazhek, 1987) gives the same results, as well as with Egyptian (Diakonoff & Porxomovsky, 1979). The split of Cushitic and Omotic (formerly `West Cushitic'), defended by, e.g., Fleming, Bender, Hetzron, etc., is not so unambiguous in view of recent comparative-historical analysis of Omotic morphology (Zaborski, 1985b; Bender, 1986).

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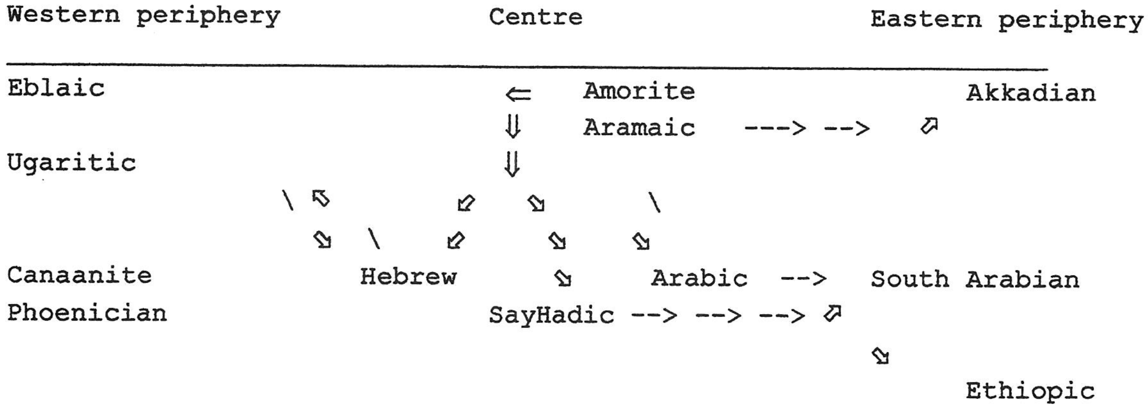
[1] Editor's Note: To avoid confusion based on the Caucasus, a world region, and Caucasian, a well-known racial term, herein we call the linguistic groups -- Caucasic. The Kartvelian group, or Georgian and its kin, often is called South Caucas-ic/-ian.

[2] Editor's Note: But cf Starostin's review of Chirikba's North Caucasic (in this volume).

[pp.92,323] Models of Semitic classification differ according to the criteria used. One of the latest classifications (by PetráDek, 1986a) is based mainly on the geographic distribution of phonological and morphological innovations and archaisms. The result is not a classical tree-diagram, but a more complicated picture registering later connections as well:



Again. different conclusions were reached by A. Militariev (1986), using glottochronological analysis:

SEMITIC:
I. NORTH: A: Akkadian, ?B: Eblaic
II.CENTRAL: A: LEVANTINE: 1) Amorite, 2) Ugaritic, 3) Canaanitic,
Aramaic
B: ARABIAN: 1) NORTH: a) Lihyan, Safaitic, T̠amudic
b) Arabic
2) SOUTH: Sabaic, Ma^{c}in, ...
C: ETHIO-SEMITIC: 1) NORTH: Geez, Tigre, Tigray[3]
2) SOUTH: Amharic, Harari, Gurage, ...
III. SOUTH: SOUTH ARABIAN

[p.92,320] The independent position of the Masa group in the Chadic family (proposed by Newman) is rejected by other authors (Jungraithmayr &

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[3] Editor's Note: Tigray in this context surely = Tigrinya. The South Arabian languages, often called Modern South Arabian, consist of Soqotri on the island and a cluster in and around Dhofar on the mainland, such as Mehri, Harsusi, Botahara, ShHauri. Not to be con-fused with the Epigraphic South Arabian languages of old, such as Sabean, Himyaritic, Qatabanian, etc.

Shimizu, 1981, p.12; Tourneux, 1987). They connect it with the Musgu and Kotoko groups, in the Riverain branch of Central Chadic.

[p.112] Ehret (1983) classifies Nilo-Saharan languages a little differently. He redefines Greenberg's East Sudanic as: I. East Sudanic (proper): a) Nilotic, Surma; b) Nuba Mountains: Temein-Jirru, Nyimang-Afitti, c) Daju; II. North Sudanic: a) Nubian, Tama, Nara (= Barea); III. Jebel: a) Berta, b) East Jebel, IV: Kuliak.

[p.116: Khoisan] The first inclusion of Sandawe in the Khoisan family was probably made by Trombetti (1910).

[p.132: Altaic] The author accepts the inclusion of Ainu in the Altaic family, following Street and Patrie (and also Ramstedt, quoted by Aalto, 1953-54, p.5). The position of the Ainu language in genetic classification is not clear, as usually in the case of languages without close relatives, since proto-language reconstruction is not possible. During the 20th century several hypotheses, besides the above-mentioned Ainu-Altaic, have been presented, e.g., by Gjerdman (1926: Ainu + Austronesian + Austroasiatic); Koppelmann (1933: Ainu + Nivx + Altaic + Uralic + Indo-European + Sumerian!); Naert (1958) and Lindquist (1960: Ainu + Indo-European); Tailleur (1961, 1966: `Paleo-Eurasian' = Ainu + Basque + North Caucasic + Kartvelian! + Burushaski + Yeniseian + Amerind!); Bouda (1960: Ainu + Nivx + Uralic), etc. The comparison of Ainu with Indo-European was rightly criticized by several authors (e.g., Tailleur, 1961; Dolgopolsky, 1963). Besides, the Ainu-Altaic proposal by Patrie (1981, 1982) is not very convincing. Patrie's best comparisons represent mainly cultural terms, while his other lexical parallels are not convincing for semantic or phonological reasons (Helimsky, 1984). The Ainu-Nivx connections are reliably analyzed as areal by Naert (1962). Only the Ainu + Austronesian + Austro-Asiatic concept of Gjerdmann (as well as Charency and Sternberg) probably has a chance to be accepted, because it is backed by very important parallels in the pronominal system, and by promising equations from the basic lexicon. (This opinion was also formulated by S.A. Starostin and I.I. Peiros. Personal communications.)

[pp.132-133, 328: Turkic] Another scheme of classification is proposed by Doerfer (Doerfer & Tezcan, 1980) (Ruhlen's nomenclature is in parentheses.):

TURKIC: I. BOLGAR: Chuvaš
II. COMMON TURKIC (I): A) KHALAJ
B) COMMON TURKIC (II)
1) OGHUZ (SOUTHERN)
2) KIPCHAQ (CENTRAL + WESTERN)
3) UIGHUR (EASTERN)
4) SOUTH SIBERIAN (TUVA-ALTAI)
5) YAKUT (together with 4. NORTHERN)[4]

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[4] Editorial Note: Because of time pressures we were unable to

[p.133] Tungus languages may also be classified differently (Vasilevich, 1960):

TUNGUS: I. MANCHURIAN: + Ju-chen, Manchu, Sibo.
II. PROPER TUNGUS: A: SIBERIAN: 1) Evenki, Solon; 2) Even, Negidal
B: LOW AMUR: Nanaj, OlDa, Orok, OroD, Udihe

[p.146, 331-333] Other patterns of Sino-Tibetan classification have recently been presented, e.g., by Yaxontov (1979):

I. CHINESE
II. WEST HIMALAYAN: Kanauri, etc.
III. CENTRAL
A) EAST HIMALAYAN: Limbu-Rai, etc.
B) NEWARI
C) BODO-NAGA-KACHIN
D) TIBETO-BURMIC: 1) TIBETAN: Tibetan, Gurung, Kaike, etc.
2) TANGUT-BURMIC; 3) KUKI-CHIN 4) ABOR-MIRI;
5) KHAM; 6) CHEPANG; 7) NUNG-TRUNG

or by Peiros and Starostin (Peiros, 1988): ⇓

I. CENTRAL: 1) TIBETO-BURMIC: (1) SICHUAN-BURMIC: (a) LOLO-BURMIC, (b) QIANG (= DZORGAI), (c) TANGUT, (d) JARUNG, (e) PUMI; (2) TIBETAN: (3) TRUNG; (4) CHEPANG, MAGARI; (5) KHAM; (6) KAIKE.
2) KUKI-CHIN: (1) CHIN, (2) TANKHUR, (3) SEMA, ANGAMI
3) MIKIR
4) KARENIC
II. EAST HIMALAYAN: Limbu, Sunwar, Thulung
III.BODO-GARO
IV. KANAURI
V. MIRI
VI. LEPCHA
VII. NEWARI
VIII. KACHIN
IX. DIGARO
X. MIDZHU (miju, mid^2 u)
XI. GURUNG
XII. CHINESE

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clarify the statement within parentheses for 5) YAKUT.

[pp.174-175: Papuan languages] It was not only J.H.Greenberg who worked simultaneously on the classification of South American and New Guinean languages; the Czech linguist C.Loukotka (1952, 1957) could also be named here.

[pp. 258-260: Large-scale groupings] The author quotes his list of the world's language families and phyla, and compares it to a similar list by Greenberg. This problem has also been analyzed by the reviewer (Blazhek, 1986). [Notes to the three schemes are indicated by small roman numerals.]:

Ruhlen (1987)	Greenberg (1987)	Blazhek (1985)
KHOISAN	KHOISAN	KHOISAN
NIGER-KORDOFANIAN	NIGER-KORDOFANIAN \	
NILO-SAHARAN	NILO-SAHARAN /	CONGO-SAHARAN
AFROASIATIC	AFROASIATIC \	
INDO-HITTITE \	|	
URALO-YUKAGHIR |	|	
ALTAIC |	EURASIATIC [i] |	NOSTRATIC [iii]
ČUKČI-KAMČATKAN |	|	
ESKALEUTAN /	|	
ELAMO-DRAVIDIAN	DRAVIDIAN |	
	SOUTH CAUCASIC /	
CAUCASIC	NORTH CAUCASIC \	
SINO-TIBETAN	SINO-TIBETAN |	SINO-CAUCASIC [iv]
NA-DENE	NA-DENE /	
AMERIND	AMERIND	AMERIND
	AUSTRO-ASIATIC [ii] \	
	AUSTRO-THAI [ii] /	AUSTRIC [v]
INDO-PACIFIC	INDO-PACIFIC	INDO-PACIFIC
AUSTRALIAN	AUSTRALIAN	AUSTRALIAN

i) Greenberg differentiates Altaic proper from Korean-Japanese & Ainu. Another member of his Eurasiatic macro-phylum is Chukchi-Eskimo, with a) Nivx or Gilyak, (b) Chukchi-Kamchatkan, and (c) Eskimo-Aleut (p.259).
ii) Ruhlen (p.153) cites Greenberg's opinion (1980) of an Austric family consisting of Miao-Yao, Austro-Asiatic, and Austro-Thai.
iii) Afro-Asiatic seems to stand in opposition to other families represented by "Micro-Nostratic" (similarly Militariev, Starostin, et al.)
iv) Including Yeniseian, Burushaski, and probably Sumerian, and perhaps Basque.
v) Perhaps including Ainu.

[p.260: Long-range connections] The author refers to other proposals of further consolidation, some of which are included in the reviewer's list above (Blazhek, 1985), e.g., Nilo-Saharan + Niger-Kordofanian (Gregersen, Boyd, Bender), Na-Dene + Sino-Tibetan (Sapir, Shafer, Swadesh). Others are still far from scientific proof, although they are not new, e.g., Indo-Pacific + Australian (Carolsfeld, 1890; Gatti, 1906-09; Trombetti, 1923, pp.63-83 [together with Dravidian]; Foley, 1986, pp.269-275).

Some proposed affiliations are questionable, e.g., where the author cites proposals connecting Dravidian with Uralic (better, directly with Nostratic), Australian, or Nilo-Saharan. He prefers, without commentary, the last possibility (because of the influence of Greenberg?). Greenberg's (1987) manuscript on Nilo-Saharan-Dravidian contains 62 etymologies. By comparison, Dravidian agrees with other Nostratic families in the following numbers of cognates. (The figures in parentheses include questionable items.) Afro-Asiatic 99 (125), Kartvelian 58 (75), Indo-European 104 (125), Uralic 105 (115), Altaic 107 (125) -- counted on the basis of 378 Nostratic etymologies (about half of Ilich-Svitych's Nostratic etymologies, including pronouns, affixes, etc.)

Dravidian-Australian connections (Caldwell, 1956, pp.75-77; Trombetti, 1923, pp.77-83; Dixon, 1980, pp. 488-489) have been discussed by the reviewer (Blazhek, 1988). He collected about 70 lexical parallels, plus other cognates in pronominal and numeral roots, concluding that they reflect relics of an "Australoid" substratum in Dravidian, rather than common heritage.

The Nostratic macro-phylum is the first case where the classical comparative method was used in the reconstruction of a common proto-language of several language families, connected only by distant relationship. Besides the Australian phylum, the following case -- Sino-Caucasic -- is another [example of the Nostratic method used for establishing a macro-phylum]. Some close connections among North Caucasic, Sino-Tibetan, and Yeniseian were suspected by Trombetti (1923, pp.201-203), and primarily by Bouda (1938, 1949, 1950a, 1950b, 1954, 1956, 1957), who collected a large number of parallels among all of the assumed members of the Sino-Caucasic macro-phylum in Eurasia. The main step in the regular establishment of Sino-Caucasic was realized by S.A.Starostin. During the seventies and early eighties he collected abundant material for the comparative dictionaries of Sino-Tibetan (with Peiros) and North Caucasic (with Nikolaev). He then elaborated new versions of the comparative-historical phonology and reconstruction of the North Caucasic, Sino-Tibetan and Yeniseian families, finally discovering the regular systems of phonetic correspondence among their proto-languages, leading to the reconstruction of a common Sino-Caucasic proto-language (Peiros & Starostin, 1977; Starostin, 1982, 1984, 1985, 1987).

Starostin's (1989) latest contribution concerns the integration of macro-phyla. He compares the reconstructed Nostratic and Sino-Caucasic proto-languages, again on the basis of the `classical' comparative method based on the establishment of a system of regular phonetic correspondences.

Because other macro-phyla have been established by the method of mass comparison, all of these conclusions are only preliminary, and they must be verified by more exact methods. It is possible that the borders between macro-phyla will be changed in the future. For example, we are not sure today whether Chukchi-Kamchatkan and Nivx -- both undoubtedly related -- belong to Nostratic (Ankeria, 1951; Bouda, 1952, 1955, 1960, 1961, 1968, 1976; Pusztay, 1979; Panfilov, 1973, etc.) or to Amerind (Sternberg, 1904; Trombetti, 1923, pp.167-189; Mudrak & Nikolaev, 1988). On the other hand, the formerly supposed connections between Sino-Tibetan and Austric have been explained as areal convergence (Peiros & Starostin, 1984).

The author does not hesitate to formulate the heretical question of polygenesis versus monogenesis. He analyzes some implications of this cardinal question, and instead of an answer he quotes Greenberg's "global etymology" *tVk- `finger/one', which may reflect common heritage from a single [Proto-]human proto-proto-language, along with other globally distributed word roots. The most industrious advocate of monogenesis was, without doubt, Alfredo Trombetti (e.g., 1905, 1906-1919, 1923, pp.189-213). A similar position is occupied by J.Rahder (1956, 1959, 1961, 1963, 1964). The rich material of mass comparisons by both authors can only be useful after verification by regular comparative methods. (Trombetti is the richest source for the Nostratic comparisons of Illich-Svitych.)

[pp.271-274: Genetic links between families] The literature can be sizably extended: cf., e.g., Gabelentz (1894: Basque + Afro-Asiatic), Woelfel (1955: Indo-European + Basque + Afro-Asia-tic), Mukarovsky (1959, 1963, 1966, 1966-67a, 1966-67b, 1972, 1980, 1981, 1987: Basque + Afro-Asiatic + Saharan + West Atlantic + Mande + Songhay), Militariev (1984a: Meroitic + Nubian + Afro-Asiatic), Hohenberger (1958: Nilotic + Cushitic + Semitic), Petráček (1986: Saharan + Afro-Asiatic), Tuttle (1932: Nubian + Dravidian), Boisson (1987a, 1987b: Sumerian + Dravidian/Nostratic), Fähnrich (1981: Sumerian + Kartvelian), Furnée (1979, 1982: Basque + Kartvelian + Burushaski + East Mediterranean substratum in Greek), etc. Some other attempts to prove distant relationships, as well as counter arguments, are discussed by Helimsky (1984, 1986a, 1986b, 1988).

[pp.302-303: Niger-Kordofanian] An alternative classification of the Mande family has been presented by Pozdnyakov (1980).

[p.310] For a more detailed classification of the North Jos group of the Plateau branch of the Benue-Zambezi stock of Niger-Congo, see Shimizu (1982).

[p.311] For a more detailed classification of the Jarawan-Bantu group, see Gerhardt (1982).

[p.317] The latest classification of the Saharan family, proposed by PetráDek (1988), follows:

SAHARAN: I) TU-KAN: (A) KAN: Kanuri, Kanembu
(B) TU: Tubu, Tuda
II) BER: (A) BERI: Bideyat, Zaghawa
(B) SAGA: Saga-toa (=Berti)

The classification of the Nubian group [p.317] is rather inaccurate: see

Thelwall (1978):

NUBIAN:
- 1) NILE NUBIAN: Mahas-Fadicca (=Nobiin), Kenzi-Dongolawi
- 2) HILL NUBIAN: (a) Dair, Kadaru-Debri, Ghulfan, El Hugeirat
  - (b) Dilling, Western Kadaru, Karko, Wali
- 3) MEIDOB
- 4) BIRGID
- 5) HARAZA †

For a more detailed classification of the Surma group, see Fleming (1983).

[p.318] For a more detailed classification of Eastern Nilotic, see Vossen (1982, p.296); for Southern Nilotic, see Rottland (1982, p.255).

[p.319] Bender (1979) lists additional languages of the Gumuz family: Disoha, Sai, Sese, Gojjam, Kokit, Hamaj.

[p.320] Militariev (1987) includes additional languages in Eastern Berber, such as Zurg (= Kufra) and Fezzan (with the varieties of Tmessa and of Al-Fojaha); in the Zenati subgroup of Northern Berber, e.g., Seghrussen, Figig, Senhaja, Iznacen, etc.; and in East Zenati: Zrawa, etc.

The Masa group belongs, no doubt, to Central Chadic (see above). Subsequently, Tourneux (1987) classifies it as follows:

1. NORTHERN: (i) MASA: Gumay, Bayga, Maraw, Walya, Bongor, Yagwa, Domo, Hara, Wina, Gizay, Bugudum
   - (ii) MUSAY (= BANANA)
   - (III) AZUMAYNA: Kolong, Marba, Lew
2. SOUTHERN: (i) ZIME: Peve, Lame, Tari (= Dari), Hede, Dzepaw (= Sachnine's Lame), Cimiang
   - (ii) MESME

[p.320-322] For a more detailed list of Chadic languages, see Jungraithmayr (1981).

[p.321: Chadic] The internal structure of the Matakam cluster is more complicated (Rossing, 1977). The Musgu cluster includes additional languages, such as Mbara, Vulum, Muskum (Tourneux, et al., 1986, pp.195-210). Similarly, Kotoko (Sölken, 1967) is a cluster of mutually unintelligible languages.

[p.322: Chadic, Omotic, Cushitic] For a full roster of Zaar (Southern Bauchi) languages, see Shimizu (1978). The Central Ometo cluster[5] is represented by such languages/dialects as Wolaita (=Wallamo), Kullo, Zala,

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[5] Central Ometo is a branch of Nomotic, of Omotic.

Gemu, Gofa, Malo, Dorze, Dache, etc. Fleming, 1976, p.300). The Central Cushitic classification is inaccurate: cf. Appleyard (1984, p.60) and Cowley (1971):

a) SOUTHERN AGAW: Awngi, Kunfäl
b) NORTHERN AGAW: (I) WESTERN: Kemant, Qwara
    (II) NORTHEASTERN: EAST: Xamir, Xamta;
        NORTH: Bilin

The Dullay cluster of languages/dialects is divided into three "branches": Harso-Dobase, Gawwada-Gollango, and Tsamay (or Gaba?) (Amborn, et al., 1980, p.56).

[p.323: Cushitic, Semitic] The following languages form the Konsoid cluster (Lamberti, 1987): Konso, Mashile, Turo, Gidole or Dirassa, Gato, Bussa or Mossiya.

It is not clear why neither Epigraphic South Arabian, nor especially modern South Arabian, are specified, while languages and/or dialects of the Gurage cluster have a place on the same page. Sabaic, Ma^{c}in, Qatabanic, Hadramautic, etc., are usually considered Epigraphic South Arabian languages, but they are not the ancestors of modern South Arabian. Arabic proper has its older epigraphic stages in LiHyan, T̲amudic, and Safaitic. The modern South Arabian languages probably represent an independent branch of the Semitic family (Militariev, 1986). Their internal classification (PetráDek, 1968; Militariev, 1984b; Simeone-Senelle, 1988) is, as follows:

SOUTH ARABIAN: I. SOQOTRI
    II.MEHRI-JIBBALI: MEHRI: Mehri, BatHari, Harsusi
        HOBYOT (AN INTERMEDIATE DIALECT);[6]
        `ŝḥERI-JIBBALI (including the
          dialect of the Kurya Murya
          islands).

[p.324] The classification of Kartvelian (South Caucasic) languages is incorrect: Svan is an independent branch (Klimov, 1961, 1971). Kartvelian is, as follows:

I SVAN, (II) ZAN-GEORGIAN: (A) Mingrelian, Laz (Chan);
    (B) Georgian

[p.325] The Hittite branch of Anatolian languages can be supplemented by Carian. Luvian is the name of two closely relat-ed, but different, languages: Cuneiform Luvian and Hieroglyphic Luvian. Lycian is also the name of two closely related languages: A and B (Milyan). Some epigraphic languages of the Hellenistic period, such as Pisidic or Sidetic, also belong to the Luvoid branch (Korolev, 1976).

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[6] Editor's Note: Hobyot was called a `transient dialect' in the original text, which we changed to `intermediate dialect' for clarity's sake. From Johnstone's descriptions Group II could be called an aging dialect cluster with few language differences.

[p.326: Iranian] The Ormuzi and Parachi languages belong to the Northwest subgroup, rather than to the Southeast (Efimov, 1986)

[p.327: Celtic, Germanic, Balto-Slavic] The classification of Celtic languages is incorrect: the Brythonic branch belongs together with Continental Gaulish (see above).

Additional tribal languages/dialects known from ancient and medieval sources belong to the East Germanic branch: Herulish, Rugish, Skirish, Gepidish. [Editor's Note: Or Herulian, Rugian, etc.]

The historical classification of the West Germanic languages is more complicated. The tripartite division known from Tacitus and Plinius is probably correct (Zhirmunsky, 1962). The mythic names relate to early medieval languages/dialects: INGAEVONIC = Anglo-Saxon, Frisian, Saxon; ISTAEVONIC = Frankish; (H)ERMINONIC = Alemannic, Bavarian, Langobardic. The modern languages were often consolidated from several sources, e.g., Dutch, from a basis of Low Frankish and Frisian; Low German, by merging Saxon and Frankish; and High German, by integration of Alemannic and Bavarian with Frankish.

Similarly, the West Baltic branch can be supplemented by such old tribal languages/dialects as Jadvingian, Galindian, Sudinian; and East Baltic by Kuronian, Zemgal, Selonian (Erhart, 1984). The recently discovered "Jatvingian" glossary reveals a remarkable Baltic language with features of both the East and West branches (Orel & Helimsky, 1987).

The traditional tripartite classification of Slavic languages probably does not reflect the real historical development. Zaliznyak (1988), for example, proposes the following model:

a) NORTHWEST: North Lexitic, Sorbian, Polish, North KriviDian
b) MIXED: (Czech), Slovak, South KriviDian, Old Novgorodia, Rostov-Suzdal dialect.
c) SOUTHEAST: Slovenian, Serbo-Croatian, Macedonian-Bulgarian, south dialects of East Slavic, Ilmen-Slovenian.

[p.328] The Yukaghir languages (traditionally "dialects" only) make up a proper language family, approximately on the same level as Samoyedic (Helimsky, personal communication), with about 2000 years of divergence, according to glottochronology. For their classification, see Tailleur (1959b, 1962):

YUKAGHIR: 1) NORTH (Tundra)
    2) CENTRAL: a † Chuvan
        b South (Kolyma); † Northwest
    3) † OMOK

There are better known Samoyedic languages. For their classification, see Helimsky (1982, p.39; 1988b, p.14):

SAMOYEDIC: 1) NORTH: Nenets, Enets, Nganasan
2) SELQUP
3) KAMASIN: Kamasin, Koibal
4) MATOR: Mator, Tajgi, Karagass[7]

Volgaic unity is questionable. For example, G.Bereczki (1982) cites 100 isoglosses between Finnic and Mordvian, against only 20 between Mordvian [sic] and Mari. A full classification of Finno-Ugric must also take into consideration the fact that some extinct languages (e.g., Merja, Muroma, known only from historical sources, onomastics, etc.) represent the joining link between Mari, Mordvian, and Finnic (Keresztes, 1987)

[p.330: Dravidian] For a more detailed classification of the Kurumba and Irula tribal languages/dialects, see Zvelebil (1985).

[p.335] For a more detailed classification of the Viet-Muong languages, see Sokolovskaya (1982)

[p.354] For a more detailed classification of the Andamanese languages, see Manoharan (1983).[8]

[p.377] The author classifies as "language isolates" the following languages: Basque, Burushaski, Ket, Nivx (=Gilyak), Nahali, Sumerian, Etruscan, Hurrian, Meroitic.

Ket is a member of the Yeniseian family, together with the extinct Arin, Assan, Kott, and Pumpokol languages (Starostin, 1982, p.144). This family belongs to the Sino-Caucasic (=Dene-Caucasic) macro-phylum (see above). Burushaski is probably also related (besides the works of Bouda, cited above, cf. also Topor-ov, 1971, and Bleichsteiner, 1930), while the Sino-Caucasic affiliation of Basque, and of Sumerian, is only hypothetical (see above). Hurrian, together with Urartian, doubtless belongs to the East Caucasic family (Diakonoff & Starostin, 1986).

The position of Nivx was discussed above. There are relatively hopeful connections between Meroitic and East Sudanic (Trigger, 1964). The oldest stratum of Nahali, determined by Kuiper (1962, 1966) shows certain common parallels with Australian (Blazhek, 1988). The genetic affiliation of Etruscan (together with the language of the Lemnos Stele) remains open: Nostratic (Kondratove & Shevoroshkin, 1970, p.145; Georgiev, 1981, pp.229-254; Gluhak, 1978, 1979) or North Caucasic (Ivanov, 1983; Orel & Starostin,

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[7] Editor's Note: Ruhlen (p.328) lists Karagas as a Turkic language, close to Tuva, in the Northern branch of Turkic. If there are not two Karagas-s, then someone seems to be mistaken.

[8] Ruhlen also lists an unpublished manuscript by Norman Zide, he of Nihali fame, on Richard Colebrooke and Andamanese.

1988)? Other "isolated" languages could also be cited, e.g., Hattic -- the old cultural language of the Hittite empire -- which is most probably related to West Caucasic (Ivanov, 1985), although some cognates with Kartvelian are also known (Girbal, 1986).

CONCLUSIONS: Despite some supplements, single corrections, and alternative schemes, this book is the best of its kind, particularly in the parts devoted to the languages of Subsaharan Africa, Southeast Asia, Oceania, and the Americas.The author works with the latest materials on the genealogical classification of languages, frequently even with as yet unpublished manuscripts. He shows the development of views on various classifications, and compares alternative opinions. This approach is new, and undoubtedly very useful, in contrast to the traditional approaches of predecessors in this field of research. This book is also more synoptical than other similar books, thanks to various diagrams, maps, and other aids.

The level of the genealogical classification of languages depends on the level of comparative-historical linguistics. It is evident that some of the data will be redefined with more precision, or otherwise changed. We hope that new editions, with fresh information (including a supplemental index of language-name synonyms) will follow.

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[Editor's Note: Ruhlen's GUIDE was essentially completed in 1985, before important Long Ranger events such as the Rice University symposium "Genetic Classification of Languages" (1986), the founding of Mother Tongue/ASLIP (1986), the publication of Greenberg's (1987) Language in the Americas, the Ann Arbor symposium "Language and Prehistory" (1988), etc. In 1991 a revised edition of the GUIDE was published, with new information and references covering many of the lacunae mentioned by Blazhek, above, as well as others not mentioned in the review.

In September, 1997, Blazhek submitted an update of his review, as follows:] [End of Editorial Note]

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Thanks to progress in comparative-historical linguistics, it is now possible to modify, or define with more precision, some of the classifications, and to supplement the references of my review (written in 1989).

CUSHITIC: My own lexico-statistical analysis does not confirm the exclusion of Beja from Cushitic. It is not possible to use the tree-diagram in classifying Cushitic languages. I prefer the "wave" interpretation, with East Cushitic representing the center of the Cushitic continuum, and Beja, Agaw, and South Cushitic (minus Dahalo) on the periphery, in the apexes of a fictional triangle. Dahalo seems to be a transitional dialect, standing midway between East Cushitic and South Cushitic.

The same approach is necessary for East Cushitic. The lexico-statistical results lead to the central position of Oromo and the Konsoid languages, while the others make up the periphery, in the following order (proximity = closer relationship): Saho-Afar, Somaloid + Baiso, Galaboid, Yaaku, Dullay, Highland East Cushitic. This solution implies a higher progressivity of the center, in agreement with the universal laws of a dialect continuum. (For example, Oromoid languages have lost the prefixal conjugation, and in Oromo all the laryngeals were dissolved.)

AFRO-ASIATIC: I am convinced that the tree-diagram is also unsuited to Afro-Asiatic. I prefer, again, the "wave" model, which can be depicted as three concentric circles. In the central position I see Egyptian (for its progressivity in the loss of prefixal conjugations, case endings, merging of the affricates, etc.). Semitic, Cushitic, and Berber occupy the central circle (=intercircle), while Chadic, Omotic, and the hypothetical pre-Sumerian substratal dialect, and perhaps Elamite, represent the oldest periphery.

CHADIC: Tourneux's (1987) arguments for the inclusion of the Masa group in Central Chadic have since been published, see H.G. Mukarovsky (1987).

SEMITIC: The classification of Petráček (19861) has since been published, as: "Pour une stratigraphie linguistique de la péninsule arabique." In <u>ŠULMU. Papers on the Ancient Near East</u>, eds., P.Vavroušek & V. SouDek, pp.257-71. Prague: Charles University.

NIGER-KORDOFANIAN: The best recent overview of Mande classification is by R.Kastenholz, "Comparative Mande Studies: State of the Art." <u>Sprache und Geschichte in Afrika</u> 12/13 (1991-92): 107-158.

In the chapter "Niger-Congo Overview" of the book <u>The Niger-Congo Languages</u>, ed., by J. Bendor-Samuel, pp.3-45 (University Press of America, Lanham-New York-London), K.Williamson presents a new classification of this macro-phylum:

I. KORDOFANIAN
II. MANDE
II. ATLANTIC-CONGO: A. Atlantic: (1) North, (2) Bijago, (3)South
    B. Ijoid
    C. Volta-Congo: (1) Kru, (2) `new' Kwa (minus East Kwa), (3) `new' Benue-Congo (plus East Kwa), (4) Dogon ?, (5) North Volta-Congo: (a) Gur, (b) Adamawa-Ubangi.

NILO-SAHARAN: The most recent classification of Nubian languages, made by M.Bechhaus-Gerst (1989, "`Nile Nubian' Reconsidered". In <u>Topics in Nilo-Saharan Linguistics,</u> M.L.Bender, ed., pp.92-93, Hamburg: Buske) is, as follows:

A. Nile Nubian: Nobiin = Mahas + Old Nobiin + Fadijja

B. West/Central Nubian: a) Central Nubian
    1) Kenzi, Dongolawi
    2) Hill Nubian
    3) Birgid
  b) Western Nubian: Meidob

The best recent overview of the classification of the Nilo-Saharan macro-phylum was presented by M.L.Bender (1996), in his book The Nilo-Saharan Languages: A Comparative Essay. Munich, Newcastle: Lincom.

SINO-TIBETAN: The most recent classification was presented by George van Driem: "Black Mountain Conjugational Morphology, Proto-Tibeto-Burman Morphosyntax, and the Linguistic Position of Chinese." Senri Ethnological Studies 41 (1995): 229-259:

A. WESTERN : Baric, Sal, Ka:maru:pan
B. EASTERN: 1) Northern: (a) Sinitic, (b) Northwestern: Bodic, Himalayan
  2) Southern: (a) Southwestern: Karen, Lolo-Burmese
    (b) Southeastern: Qia:ngic, Rung.

DENE-CAUCASIC: The Sino-Caucasic hypothesis has been supported, besides Bengtson's numerous articles [see the partial list provided by R.L.Trask in Mother Tongue I, p.79], also by Blazhek & Bengtson, "Lexica Dene-Caucasica," Central Asiatic Journal 39.1: 11-50, (Addenda-Corrigenda in 39.2: 161-164).

DRAVIDIAN and AUSTRALIAN: My article (Blazhek, 1988) was published in the book Nostratic, Dene-Caucasian, Austric and Amerind (Shevoroshkin, 1992), pp.421-431.

AUSTRALIAN: Important changes in the classification of Australian languages were published by B.J.Blake (1988), "Redefining Pama-Nyungan: towards the prehistory of Australian languages," In Aboriginal Linguistics, ed., by N.Evans & S.Johnson, pp.1-90. Armidale; and by N. Evans, "Arguments for Pama-Nyungan as a genetic sub-group, with particular reference to initial laminalization," ibid., pp.91-110.

ABBREVIATIONS:

AuÜ	Afrika und Übersee
BPX	Bochum Publication in Evolutionary Cultural Semiotics
GLECS	Comptes rendus du Groupe linguistique d'études Chamito-Sémitiques.
HS-5	Papers presented at the 5th International Hamito-

	Semitic Congress, Vienna, Sept.28-Oct.2, 1987.
JAOS	*Journal of the American Oriental Society*
LP	Paper presented at the symposium "Language and Prehistory," University of Michigan, Ann Arbor, Nov.8-12, 1988 [Several papers and abstracts were subsequently published in the BPX series: see Shevoroshkin, (ed.) 1989a,b,1990, 1991, 1992.]
LRDIV	*Lingvisticheskaya rekonstruktsiya i drevneyshaya istoriya Vostoka*. 1984. Moscow, Nauka.
MKNA	*Mededelinger der Koninklijke Nederlandse Akademie van Wetenschappen,* Afd. Lett., N.R.
WZKM	*Wiener Zeitschrift die Kunde des Morgenlandes*
ZDMG	*Zeitschrift der Deutschen morgenländischen Gesellschaft.*

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BOOK REVIEW ARTICLE

Common West Caucasian: The Reconstruction of its Phonological System and Parts of its Lexicon and Morphology (CNWS Publication, No. 48), by Viacheslav A. Chirikba. Leiden: Research School CNWS, 1996. xxvi + 452 pages.

Reviewed by Sergei A. Starostin, Russian State University for the Humanities, Moscow.

[Publishers Résumé: This book deals with the reconstruction of Common West Caucasian, the postulated proto-language of the West Caucasian (Abkhazo-Adyghean) languages: Abkhaz, Circassian, and the recently extinct Ubykh.

The book contains a synchronous description of the phonetic systems of all West Caucasian dialects, and presents intermediate reconstructions of the Proto-Abkhaz, Proto-Circassian and Proto-Ubykh phonemic systems.

The reconstructed intermediary proto-languages serve as a basis for the reconstruction of Common West Caucasian. Besides phonology, the book also deals with certain aspects of the morphology (nominal and verbal affixation, ablaut) and the lexicon of Common West Caucasian.

Finally, the author touches upon the problem of the external relations of Common West Caucasian, namely, with the East Caucasian (or Nakh-Daghestanian) languages, and the long extinct Hattic language of ancient Asia Minor (early second millennium B.C.).]

EDITOR'S NOTE:

The following review article was received from Professor Starostin via e-mail. Due to the extreme range of phones and symbols for them, inherent in West Caucasic languages and the literature about them, we have left the original text wholly untouched - out of fear of distorting things about which we are far from expert, including the electronic technology's out-put and the formidable phonetics in tandem with the ideosyncratic Russian transcription. **HF**

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S. A. Starostin

A review of V. A. Chirikba's "Common West Caucasian"

This is a very important book, in fact, starting the discussion of Proto-West-Caucasian (PWC) on an entirely new level. It contains abundant material from all West Caucasian languages and a consistent reconstruction of Common Abkhaz, Common Circassian and Common West Caucasian phonological systems.

In this review I shall dwell mainly upon the differences between Chirikba's reconstruction, and my reconstruction of Common West Caucasian (CWC) presented in NCED. I shall try to be objective and acknowledge valuable additions and emendations. I am ready, however, to defend several aspects of the CWC reconstruction presented in NCED and ignored or challenged by the author.

1. Reconstruction of Common Abkhaz.

The Common Abkhaz system of consonants reconstructed by Chirikba virtually coincides with the system presented in NCED, except for two additions (*ṗ^{w} and *v) and one elimination (*ʡ).

I would agree with the reconstruction of *v: although this phoneme is reconstructed only in one root (**vǝrǝ-vǝrǝ* 'sound of swiftly turning round'), the root is present in several Abkhaz dialects and fills a slot available in the system. There are also other cases of phonemes attested only within a single root in North Caucasian languages - not surprising because of their huge consonantal inventory.

The reconstruction of labialazed *ṗ^{w} is more dubious: this phoneme is also reconstructed only on the basis of one root (-**ṗ^{w}ə̄* in **ša-ṗ^{w}ə̄* 'foot', **na-ṗ^{w}ə̄* 'hand'), and its reflexes differ from those of *ṗ only in one dialect (Ashkharywa). Especially strange is the fact that CA does not possess any other labialized labial consonants (there are no *p^{w} and *b^{w} either in my reconstruction or in Chirikba's). Therefore, I strongly suspect that the variant *šaṗ^{w}ə̄, mṗ^{w}ə̄* in Ashkharywa (note that *šaṗə̄* and *mṗə̄* are also attested) reflects just a positional articulation of ṗ in a final stressed syllable with a mid vowel -ə̄.

My reconstruction of *ʡ in CA was based on the specific correspondence of Tapanta ʡ to q̇ in other Abkhaz dialects (in clusters *ʡ sometimes gets lost). There are several roots demonstrating this correspondence:

SAbkh *a-maq̇á* : Tap *m(a)ʡa* 'girdle, belt'
SAbkh *-da* : Tap *-dʡa* 'without' (a privative suffix)
SAbkh *zaq̇á* : Tap *zʡa-rá* 'how many'
SAbkh *á-šaq̇a* : Tap *šʡa* 'pillar, stanchion'
SAbkh *a-š^{v}q̇ə̄* : Tap *š^{w}ʡa* 'letter, document'
SAbkh *-q̇a* / *-a* : Tap *-ʡa* 'a locative suffix'

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SAbkh *-q̇a-* : Tap *-ʡa-* ’to be’.

V. Chirikba suggests that Tap. ʡ may be a positional variant of q̇, but no rules of positional distribution are given. I, therefore, can not agree with the elimination of *ʡ from the CA system. Chirikba’s objection that ʡ is present only in Tapanta is not quite clear to me: there are lots of phonemes in CA reconstructed only on the basis of one dialect (for example, all palatals - *ć, *ḉ̣, *ś, *ź - are only preserved in Bzyp, and still are clearly reconstructable for CA).

V. Chirikba reconstructs a number of consonant clusters for CA, pointing out (p. 76) that ’in principle, correspondences of consonants in clusters are practically identical with those of single consonants’). This is another difference from the system presented in NCED, where no consonant clusters are reconstructed for PA.

As a matter of fact, it is rather easy to show that the mid vowel -ə̄- in Abkhaz can only exist in a stressed position, and is lost in an unstressed syllable: cf. *á-pšᵛma* ’host’ < **á-pə̄šᵛə̄ma* vs. *apšᵛə̄ma-ḳ* ’one host’ etc.). This rule is violated only in a few cases when it leads to formation of phonetically illicit consonant clusters (cf. *á-ħʷə̄źba* ’knife’; sometimes a free variation -ə̄- / -0- occurs in such cases, cf. *a-ħʷnḉ̣ᵛá* along with *a-ħʷə̄nḉ̣ᵛá* ’dirt’ etc.). Therefore, all consonant clusters in Abkhaz can in fact be analysed as a result of -ə̄-elimination (sometimes with following assimilations) - which is what was done in NCED. The only possible source for reconstructing real clusters in CA could be cases of the correspondence CC : C in Abkhaz dialects (like SAbkh *á-bʕʷa* : Tap. *ʕʷa* copper etc., see the list in Chirikba 77). These cases, however, are too few and unsystematic, and can not be regarded as anything other than late dialectal variations.

2. Reconstruction of Common Circassian (CC).

Here Chirikba basically follows the classical reconstruction of Kuipers (Kuipers 1963). His own major addition is the reconstruction of CC stress patterns in disyllabic words, based on his own field recordings of Temirgoj and Abadzakh stress (pp. 166-170), and coinciding rather neatly with the reconstruction obtained by S. Nikolayev (see NCED 198) solely on the basis of vocalic correspondences between disyllabic roots.

3. Reconstruction of the Common West Caucasian (CWC) system.

This is the part of the book that contains most differences between Chirikba’s and my reconstructions. Some preliminary remarks are needed.

The set of consonants reconstructed by Chirikba for CWC is actually a subset of the system presented in NCED. He expresses "serious doubts as to the reliability of such a huge phonemic inventory, reaching a total of 168 consonants" (p. 12). His own system, however, reaches 110 consonants (see the chart on pp. 174-178), which is still much more than in any living language. The problem with consonant systems of the WC type is actually not the abundance of consonants: it is the almost algebraic distribution of distinctive features within the system. Thus, the features of palatalization and labialization are perfectly well known and widely spread in the languages of the world: but a complete distribution (plain consonants : palatalized consonants : labialized consonants : palatalized labialized consonants) is fairly rare. In fact, in such a system one can think of additional distinctive features (palatalization, labialization, pharyngealization) as a sort of phonemes (say, /J/, /W/, /H/) - which will at once drastically reduce the number of basic consonants. Thus it is easy to see that a big number of consonants (be it 168 or 110) in itself is not a theoretical obstacle of any kind - it is just a problem of phonological interpretation.

I would also counter two other objections raised by Chirikba on p. 12:
a) "pharyngealization and palatalization are always in complementary distribution in any language where they occur within one phonological system". This is an old thesis belonging to N. S. Trubetzkoy [Trubetzkoy 1931] and due to his incomplete knowledge of Caucasian consonant systems at that time: see my comments to his paper [Starostin 1987, pp. 465-466, 472]. We have, for example, the now well described Tsakhur language having both pharyngealization and palatalization coexisting within one system and quite independent from each other.
b) "the authors, as much as Abdokov, retort nearly exclusively to the method of external reconstruction, which strongly determines the shape of the whole of Proto-West Caucasian reconstruction". This is probably some misunderstanding on Chirikba's part: the whole CWC reconstruction presented in NCED was based solely on internal WC evidence, without any influence of Eastern Caucasian parallels. It is true, of course, that when internal WC data allow for several reconstructions in CWC, we choose the one that fits best into the particular North Caucasian reconstruction - but this is the usual practice of comparative linguistics. Chirikba maintains that "sometimes the internal analysis of a given word can present a more simple and economical explanation". This, of course, can be true in some particular cases, but the remark is rather general, and deserves a similarly general response: any internal reconstruction should be checked against external evidence (if such evidence exists), and from the scientific point of view the best situation is when internal and external evidence match. Also, the "more simple and economical explanation" unfortunately is not always the true one, which is well known to every comparative scholar.

Before I proceed with my arguments for every individual case of discrepancy between my system and that of Chirikba's, I must point out that the solution he proposes to do away with 58 excessive consonants in CWC is rather simple: Chirikba declares that pharyngealization in Ubykh is secondary, and the opposition "lax-tense" in Adygh is also secondary. Let us try to understand the author's arguments.

1. Pharyngealization.

Among WC languages, pharyngealization is attested in Ubykh only. Chirikba abstains from projecting it to the CWC level for two reasons (p. 333):
a) "firstly because the reconstruction of pharyngealised consonants, added to the already reconstructed plain, palatalised, labialised and labialised-palatalised series, will lead to a substantial increase of the already huge consonantal inventory, which renders it typologically improbable". This seems obviously an invalid argument. We can not violate the system of correspondences just because we do not want the system to become too abundant: we must have a way to "explain away" the Ubykh pharyngealized consonants (e. g., demonstrating that they go back to some clusters, or arise out of some suprasegmental features etc.). The sheer desire to make the reconstructed system smaller is not enough: in a similar way we could do away, e. g., with the palatalised-labialised consonants, or, in fact, any other group or series of consonants.
b) "the correspondences of the Ubykh pharyngealised consonants to their counterparts in sister-languages do not form a special set, different from correspondences with Ubykh non-pharyngealised consonants". If this were true, this would be indeed a serious argument for dismissing the CWC pharyngealised consonants. Unfortunately, this is not true at all. Pharyngealisation was indeed lost without a trace in Abkhaz and Adygh in the case of labial consonants; but uvular pharyngealized consonants in most cases give different reflexes, compared with non-pharyngealized ones. All cases like that are declared by Chirikba to be "irregular" (see below), which is of course the easiest way. On page 333 he himself, however, lists several cases like that, in a rather informative passage:

"Though there are enough reasons to regard the pharyngealised consonants in Ubykh as an innovation, still their existence in Ubykh could in principle indicate their earlier presence as a feature of the vocalic system, which later disappeared in Proto-Circassian and Proto-Abkhaz, but shifted to the consonants in Proto-Ubykh." This means that Chirikba, in fact, acknowledges the reconstruction of pharyngealization but prefers to treat it rather as a vocalic than as a consonantal feature. This, again, is purely a matter of phonological interpretation. I have favoured the reconstruction of pharyngealized consonants because of the restricted distribution of pharyngealization: it is present only in the labial and uvular local series, which is typical for systems with pharyngealized consonants and not vowels (in the latter case we would expect a wider distribution of pharyngealization, not depending on the nature of neighbouring consonants) - see the discussion in Starostin 1987.

2. Opposition "Lax - tense"

Proto-Circassian has a whole set of tense stops and fricatives (*p:, *t:, *c:, *š: etc.). Some of them regularly correspond to Abkhaz voiced consonants, others - to Abkhaz voiceless (see a complete account in NCED and below). For a reason of his own (basically - in order to diminish the number of phonemes in CWC) Chirikba dismisses the former type of correspondences as irregular, but acknowledges only the second set, considering tenseness in Circassian to be always secondary. Even if we follow Chirikba and regard the correspondences "PC tense : PA voiced" as secondary, we are still left with the split of voiceless consonants into lax and tense in PC. Chirikba does not even attempt to give any positional conditions for this split and this solution is clearly unsatisfactory from a methodological point of view. On the other hand, the system of correspondences proposed in NCED and presuming the archaic nature of Circassian tense consonants allows to regard a great amount of cases as quite regular, without any need for assuming any arbitrary splits.

Labial stops

The PWC stops *b, *p, *ṗ are in general reconstructed correctly. For *p, however, the Circassian reflexes are given as p, p:. But the only example of PC *p: given is PC **p:a* 'to be out of breath': Ub. *pə̄-* : Tap. *-pə̄-* 'to grieve, be annoyed' (p. 180). Chirikba quotes Kuipers 1975 with Bzhed. *p:a-*, but gives also Ad. *zag*^{W}*a-pa-* 'to be angry'. These are, in fact, two distinct PC roots, the Bzhed. form for 'to be angry' being also *zag*^{W}*a-pa-* - see Kuipers 1975, 10. There is, therefore, not a single example of the development PWC *p > PC p: !

On the other hand, there are several cases of a (perfectly regular) correspondence "Abkh., Ub. b : PC p:" (see NCED 180). These are:

1) Abkh. **bə̄na* "wood", Ub. *baná* "grass" : PC **p:aná* "thorn" (assumed by Chirikba to be irregular, see p. 335)
2) Abkh. **bə̄rə̄c*^{W}*ə̄* 'medlar', Ub. *bə̄rə̄c*^{V}: PC **p:ə̄ra-źə̄ja* 'sloe' (same remark)
3) Abkh. **saba* 'dust' : PC **sáp:a* (Ad. *sāpa* > Ub. *sāpa*).
4) Ub. *ba-q́la* 'enemy' : PC **p:ə̄-jə̄* id.
5) Abkh. **bə̄t*^{W}*a* 'spring wool' : PC **p:acá* id. (see below for the explanation of the Inlaut correspondence)

Some specific cluster correspondences (also declared irregular on p.337) are also easily explained if we assume the original nature of PC *p:, cf.

6) Abkh. **pə̄śə̄-* 'fish' (assimilation < **p:ə̄śə̄-*), Ub. *psa* (similar assimilation) : PC **p:c:a* (progressive assimilation) < PWC **p:ə̄šV*
7) Abkh. **-mə̄sa* 'eyebrow' (< **bə̄sa*), Ub. *-msa* (similar development) : PC **-p:c:a* (progressive assimilation) < PWC **p:ə̄sa.*

More cluster examples can be given, but the above, I think, is enough to show that the correspondence "Abkh., Ub. b : PC p:" is regular, while the correspondence proposed by Chirikba ("Abkh., Ub. p : PC p:") does not exist.

On p. 178 Chirikba says that he "does not find enough evidence which would indicate the presence of the labials' labialised, palatalised or labialised-palatalised correlates". As a matter of fact, there is some evidence in favour of those consonants - although not abundant (but there is scarcely any abundant evidence for any PWC phoneme, just because of their great number). I mean several cognates involving correspondences between Ub. f and labial stops in other languages, as well as a number of cognates involving correspondences of labial stops and dental labialised consonants or palatal affricates.

a) Labialized labials

Labialized *p^{w} and *b^{w} can be reconstructed in several cases where Ubykh has the fricative f, while Abkhaz has either *p (< *p^{w}) or *f (< *b^{w}) and PC has *p, *b. Cf.:

1) Ubykh _fa-,_ Abkh. _*pə̄-_ : PC _*pa_ 'nose' < PWC _*p_^{w}_V_
2) Ubykh _fə̄-mə̄-_ 'to smell', Abkh. _*fə̄-ʕ_^{w}_ə̄_ 'smell' : PC _*ba-má_ 'smell' < PWC _*b_^{w}_V_
3) Ubykh _fawə̄-_ 'to fight' : PC _*bana-_ id. < PWC _*b_^{w}_V̆_

All three examples seem phonetically and semantically quite reliable, and the development *p^{w}, *b^{w} > f is easy to explain. I must note, however, that no tense (*p:^{w}) or glottalised (*ṗ^{w}) labialised labial stops can be reconstructed.

b) Palatalized labials.

Palatalized labials do not exist in modern WC languages. There is, however, a peculiar set of correspondences where Abkh. labials correspond to Ubykh dental labialized and Circassian dentals. According to phonetic descriptions, Ubykh dental labialized stops were pronounced similarly to the same phonemes in Abkhaz, i. e. basically as dentals with stopped labialization and pronounced palatalization (i. e. /db́/, /tṕ/, /ṭṕ/); in Abkhaz I have witnessed this articulation myself in several dialects. It is easy, therefore, to suppose a development like *ṕ > /tṕ/ (= t^{w}) etc. At the present time I see no other possible explanation of these rows of correspondences (one can not reconstruct anything like *t^{w} here - just because perfectly normal correspondences exist for *t^{w}, *d^{w} etc.). The examples in question are:

1) Abkh. _-ṗ_ 'suffix of present / future tense in verbs' (probably < _*-pə̄_ with secondary glottalisation in the specific auxiliary function on the end of verbal forms) : Ub. _t_^{w}_-_ 'to be' : PC _*-tV-_ 'to be' < PWC _*ṕə̄-_. [PC _*-tV-_ can reflect a merger of two different WC roots - _*ṕə̄-_ and _*tə̄-_ 'to be', the latter reflected also in Abkh. _*ta-_ and Ub. _-tə̄-;_ but the Abkh. - Ubykh parallel still points to PWC *ṕ].

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2) Abkh. *pa 'to jump; to copulate (of animals)' : Ub. t^{w}a- id. < PWC *ṕa
3) Abkh. *pa 'son; male' : Ub. t^{w}a- 'male' < PWC *ṕa
4) Abkh. *abə̄ 'father' : Ub. t^{w}ə̄ : PC *at:a id. < PWC *(a)ṕ:ə̄

Note that in this case again we have a (quite normal from my point of view) correspondence of a tense consonant in Circassian to a voiced consonant in Abkhaz, pointing to a tense stop in PWC.

5) Abkh. *ajə̄-ba- 'reciprocal prefix' : Ub. -d^{w}a 'distributive affix' : PC *da- 'reciprocal, collective prefix' < PWC *b́a
6) Abkh. *baba 'soft, shaggy': Ub. d^{w}ə̄d^{w}á 'fluffy, downy' < PWC *b́Vb́a
7) Abkh. *ṗə̄ 'to dig making the ground light by crumbling it' : Ub. ṭ^{w}ə̄'to dig' : PC *ṭa- id. < PWC *ṗ́ə̄ [Chirikba 182 compares the Abkh. form with PC *wə̄-ṗə̄-č̣ḳ^{w}ə̄- 'crush, rumple', Ub. ǵə̄-ṗa- 'torment, torture'which, I think, is less convincing].

Some of these examples may be disputed, but the general pattern is quite consistent, and I can not agree with Chirikba (p. 189) who just declares this to be an "irregular correspondence".

c) Palatalized labialized labials.

There are several cognates where Chirikba (pp. 186, 189) reconstructs dental palatalized labialized stops (*d́^{w}a 'to sew', *d́^{w}a-d́^{w}ə̄ 'awl', *t́^{w}a 'cherry'). These are the rows of correspondences where I (see NCED 181) reconstruct PWC labial palatalized labialized *b́^{w}, *ṕ^{w} (there are also some forms where *ṕ^{w} can be reconstructed, but they are more dubious). There are actually two reasons for such a reconstruction in NCED:

a) there exists another group of examples (ignored by Chirikba), where Abkhaz and Ubykh labialized dental stops correspond to Circassian affricates and where I reconstruct PWC dental palatalized labialized stops, cf.:

1) Ub. ṭ^{w}a- 'pus, suppurate' : Kab. ç̣ə̄-na 'pus' < PWC *ṭ́^{w}V
2) Ub. mṭ^{w}a (with a variant mṗa) 'lead' : PC *p:c:a- : Abkh. *pə̄ṭə̄-sa (with a dissimilation < *pə̄ṭ^{w}ə̄-sa), cf. Abzh., Tap. á-ṭsa, Bzyp. á-psta < PWC *p:ə̃̄ṭ́^{w}V (note the regular progressive assimilation in Circassian: *p:ə̃̄ṭ́^{w}V > *p:ç̣a > *p:c:a)
3) PC *p:acá 'spring wool' : Abkh. *bə̄t^{w}a id. < PWC *p:Vt́^{w}a.

b) theoretically both groups of examples can be interpreted on WC ground either as dental or labial stops; both groups certainly point both to labialization and palatalization in PWC. However, in the first group the root *b́^{w}a (Chirikba's *d́^{w}a) corresponds to PEC *=ĭrbV 'to sew' (see NCED 648-649), while in the second one PWC *p:Vt́^{w}a seems to correspond to PEC *pɨ ˘dwV / *bɨ ˘twV 'feather, down' (NCED 874).

This is exactly one of the cases where external evidence helps to solve otherwise obscure internal WC problems, pointing to the probablity of reconstructing labial (palatalised labialised) consonants in the first group and dental (palatalised labialised) consonants in the second one.

Of course, timbred labials were rare in PWC; still, the examples presented above seem to fill an empty slot in the system and reduce the number of irregularly corresponding cognates. It is interesting to note that timbred labial stops occur almost exclusively in monosyllabic (or reduplicated) roots: this means probably that in polysyllabic roots the phonetically peculiar palatalized / labialized labials were lost earlier, even before the split of Common West Caucasian.

Dental stops.

For PWC *t, *d, *ṭ, *tʷ, *dʷ, *ṭʷ the reconstruction presented in NCED and in Chirikba's book is generally the same. Again, however, Chirikba proposes a secondary split *t > PC *t, *t:; *tʷ > PC *t, *t:. The examples he lists for PC *t: < *t, *tʷ are:

1) PC **ma(r)t:a* 'quantity, measure' : Abkh. **mata* 'a strip of land which has to be ploughed or hoed'
2) PC **at:aq:a* 'cock, rooster' : Ub. *taqa* id.
3) PC **sʷt:ə̄* 'to hurl' : Abkh. **sʷə̄tə̄* 'to overturn; to rush'
4) PC **wə̄-t:a* 'to pound, thresh' : Ub. tʷā̆tʷa 'green woodpecker', *tʷatʷa-wə̄* 'arms, weapons', Abkh. **tʷatʷa* 'to husk the grain out of ears'
5) PC **pśa-t:ə̄q:ə̄* 'back of neck, occiput' : Ub. *tʷə̄q* 'neck'

While the first example is rather dubious semantically, the others are fine. Note, however, that:

a) PC **sʷt:ə̄* 'to hurl' (Kuipers 1975, 16) is reflected only in Shapsug and Beslenej and can be in fact reconstructed as **sʷtə̄-* (Shaps. *jə̄-sᵛtə̄-*, Besl. *jə̄-ftə̄-;* no forms with a reflex of *t: are present).
b) In No 4 a better Abkh. match seems to be **dʷə̄dʷə̄-* 'to grind roughly' (Chirikba compares it with Ub. *dʷə̄dʷa-* 'fluffy', but for the latter an obvious Abkhaz match is **baba* 'soft, shaggy', see above);
c) In No 5 a very good (although metathesized) Abkh. match is **qʷə̄da* 'neck' with a voiced *d.

We, therefore, arrive at a very clear system of correspondences:

PWC *t: > Abkh. *d, Ub. t, PC *t: [unfortunately, in NCED 181 there is a misprint

and the Ubykh reflex is listed as d - although in the body of the dictionary the correspondence is as stated, cf. _*dVGwV_ 'cock' etc.]
PWC *t:^{w} > Abkh. *d^w, Ub. t^w, PC *t:.

Here, just as in the system of labials, there seems to be enough ground for reconstructing separate tense dental stops *t:, *t:^{w}. Note that they behave just like *p: in Abkh. and Circassian, but yield voiceless reflexes in Ubykh (the same is true for palatalized *ṗ: in the root for 'father', see above).

Velar stops

In general, the reconstruction presented by Chirikba coincides with the one presented in NCED. This is true for most reconstructed velars: *g, *ǵ, *g^w, *ǵ^w, *ḳ, *ḳ́, *ḳ^w, *ḳ́^{w}. But again, tense *k: in PC stays unaccounted for. Chirikba here, as in other cases, considers PC *k: to be a secondary development of *k, postulating:

a) PWC *k > PC *k / *k:
Here the only example of PC *k: is a very dubious match between Abkh. _*kada_ 'side' and PC _*k:áp:a_ (where -d- and -p:- are absolutely unclear);

b) PWC *k^w > PC *k^w / *k:^{w}
The only examples of *k:^{w} here are PC _*k:^{w}ə̄śa_ 'cradle' : Abkh. _*k^{w}aśə̄-k^{w}aśə̄_ 'to mince, go at a jog-trot' (??) and PC _*k:^{w}ə̄rV-k:^{w}ə̄rV_ 'call for turkeys' : Abkh. _*k^{w}ə̄rə̄-k^{w}ə̄rə̄_ 'to neigh' (??). I will not comment the validity of those examples.

No examples of *ḱ > PC *k: or *ḱ^w > PC *k:^{w} are given at all.

As a matter of fact, PC *k: is a very rare phoneme (only seven examples in Kuipers 1975), and I indeed do not know any secure examples for PWC *k:, *ḱ: or *ḱ:^{w}. One word with *k: (PC _*k:at:ə̄_ 'hen') has a good parallel in Abkh. _*ḳ^{w}ə̄ṭə̄_, but this root most certainly underwent assimilations: the PWC form must be reconstructed as _*k:^{w}Vṭə̄ // *k:Vṭ^{w}ə̄_ (with _*k:^{w}Vṭə̄ > *ḳ^{w}ə̄ṭə̄_ in Abkh. and _*k:Vṭ^{w}ə̄ > *k:at:^{w}ə̄ > *k:at:ə̄_ in PC; see NCED 444, PNC _*gwāṭa_). Two words with *k: in PC (_*k:ap:á_ 'side' and _*k:ə̄-jə̄_ 'gullet') have parallels in Eastern Caucasian (see NCED 292, 431), but unfortunately lack closer parallels in Western Caucasian. It seems that PWC *k: and *ḱ: were rare, just as their reflexes in PC.

However, we have rather good evidence for PWC *k:^{w}. Consider the following examples:

1) PC _*k:^{w}árta_ 'flock of sheep' : Abkh. _*g^{w}arə̄ta_ id.
2) PC _*k:^{w}ə̄pə̄_ 'group' : Abkh. _*g^{w}ə̄pə̄_ id. 3) PC _*-k:^{w}a_ 'a privative or pejorative

suffix' (in **c:a-k:ʷá* 'blunt', lit. 'toothles' or 'with bad teeth', **bza-k:ʷá* 'dumb', lit. 'tongueless' or 'with bad tongue') : Abkh. **-gʷV* id. (in **ca-gʷV* 'blunt', **ḳ́a-gʷV* // **ḳ́aṭa-gʷV* 'short-tailed') : Ub. *gʷə̄, agʷə̄́* 'small'.

Chirikba does not cite the third example (present in literature, see the discussion in Shagirov 1, 163), and regards the first two examples as "not very reliable from the point of view of their genuine character" (pp. 335-336). This seems rather strange to me, because the quality of these examples seems to be certainly better than the obviously onomatopoetic matches for **k:ʷaśə̄* and **k:ʷə̄rə̄-k:ʷə̄rə̄,* and the obscure match between **k:áp:a* and **kada* presented above.

The velar series, therefore, also demonstrates the consistent pattern of Abkhaz voiced corresponding to Adygh tense consonants, seen throughout the whole system of stops.

Uvular stops.

A preliminary note: I would rather speak of uvular affricates, not stops. Uvular consonants (perhaps, with the exception of voiced G and tense q:) are usually articulated as affricates in most Caucasian languages, not excluding Western Caucasian.

The reconstruction of uvular stops in Chirikba 208-222 is quite different from the system presented in NCED, and I must dwell on it in some detail. The basic reason for the different interpretation is the fact that Chirikba does not want to acknowledge the genuine nature of Ubykh pharyngealization and is therefore forced to suppose numerous unmotivated splits in the development of individual phonemes.

I shall start here with the system presented in NCED for plain uvulars:

1. PWC *q > Abkh. *ħ, Ub. q, PC q
PWC *q: > Abkh. *q, Ub. q, PC *q:

PWC *q is a rare phoneme, reflected in:
PWC **qa* > Ub. *qa,* PC **qa* 'grave', Abkh. **ħa-pə̄* 'cave'
PWC **qa* > Ub. **qa* 'little pimples', 'chaps', PC **qa-(m)ṗa* 'dandruff', perhaps also Bzyp *á-ħa-mp* 'layer of fern under the reed roof'

Chirikba lists both examples (p. 210) without Abkhaz counterparts, but adds also several cases with PC *q: (considering it to be, as usual, a secondary development):

Ub. *taqa,* PC **at:aq:a* 'rooster'
Ub. *qə̄,* PC **q:ə̄* 'to be benumbed'

Ub. *tʷə̄q*, PC **t:ə̄q:ə̄* 'neck' [I would also add Abkh. **qʷə̄da* 'neck', see above]
PC **wə̄-q:ə̄* 'to distend', Abkh. **qa* 'to pull'

One final example is Ub. *žʷaqa* 'late': Abkh. **aqa* 'night' (without a Circassian parallel).

I can also add the following example:

PC **q:ə̄* 'big' (Kuipers 1975, 64) : Abkh. **qa* 'enough, be enough'.

From the above examples it seems quite obvious that:

a) Abkh. *q does not correspond to PC *q - there is no single example of this match;
b) whenever PC has *q:, Abkhaz has *q, and in the few matching cases where PC has *q, Abkhaz has *ħ.

It seems quite evident to me that Chirikba (with his dislike for tense consonants) confuses here two rows of correspondences:

a) *q > Abkh. *ħ, Ub. q, PC q
b) *q: > Abkh. *q, Ub. q, PC *q:

Here, as well as in most of other affricate series, PWC tense consonants yield Abkhaz and Ubykh voiceless reflexes, as opposed to the system of stops.

2. PWC *G > Abkh. *ɣ, Ub. ɓ, PC *ɓ

This is a row of correspondences which is very numerous and, of course, acknowledged by Chirikba. However, he reconstructs here a fricative *ɓ, not a stop or affricate (see pp. 281-282).
The reasons why I have reconstructed a stop (affricate) where all the daughter-languages have fricatives are the following:

a) phonetically G is a rather rare and unstable phoneme in most Caucasian languages and it tends to become fricativized very easily;
b) I reconstruct a PWC *ɓ for the row of correspondences Abkh. *ʕ : Ubykh ɓ, PC *ɓ. This row of correspondences is also present in Chirikba's book (pp. 289-290), but is treated here as PWC *ʕ.

Now the natural objection against reconstructing *ʕ for Abkh. *ʕ : Ubykh ɓ : PC *ɓ is the fact that in no language I know do the pharyngeals [from the phonetic point of view I would avoid the term 'pharyngeals' and speak, after S. Kodzasov, about emphatic laryngeals] ever yield uvular fricatives - whereas the opposite development (from *ʁ to ʕ or from *χ to ħ) is attested quite abundantly.

The obvious reconstruction for the two discussed rows of correspondences seems to me:

*ʁ > Abkh. ʕ, Ub. ʁ, PC *ʁ
*G > Abkh. ɣ, Ub. ʁ, PC *ʁ.

We see that *ʁ and *G have merged in PC and Ubykh (which is, as I said, a quite normal phenomenon), but are kept distinct in Abkhaz - also with a perfectly well explainable shift *ʁ > ʕ followed by *G > *ʁ (= *ɣ). Note that the notation *ɣ for Proto-Abkhaz actually means a uvular *ʁ, because the opposition of velar *ɣ and uvular *ʁ is lacking both in Common Abkhaz and in all modern dialects.

3. PWC *q̇ > Abkh. *q̇, Ub. q̇, PC *q:.

This row of correspondences is observed in the following items:

PWC **q̇a-* 'affix in colour designations' > Abkh. **q̇a-* in **q̇a-pə̄šə̄* 'red';
Ub. *-q̇a* in *ʁIa-q̇á* 'blue, green'; PC **q:a-* in **q:a-šχʷa* 'blue, green'.
PWC **q̇a-* 'blunt' > Abkh. **q̇a-gʷa*, PC **q:a-* 'be blunt, benumbed'
PWC **q̇V-* 'cut' > Abkh. **q̇V-*, Ub. *q̇ə̄-*. Chirikba (following Abdokov) compares also PC **wə̄-ʔa-* 'to wound', but I would rather compare a semantically closer PC **q̇a-má* 'dagger' (= 'cutting tool', formed like **wə̄-má* 'big wooden hammer' = 'striking tool' with the instrumental suffix *-ma and widely borrowed into neighbour languages). For some reason Chirikba lists one and the same root under numbers (2) and (10).
PWC **q̇V-* > Abkh. **q̇ə̄-* 'to damn', **q̇a-tʷa-* 'to inflict damage', Ub. *La-q̇a* 'damage, evil' (Chirikba 217)
PWC **ṗq̇V-* > Abkh. **pq̇ə̄-źə̄, *ṗq̇ə̄-šə̄* 'reed', Ub. *ṗq̇a-č̣a* 'reed' (ibid.)

Chirikba (pp. 208, 217) has instead two different correspondences:

a) Abkh. *ʕ : Ub. q̇, q̇I : PC *q: < PWC *G

Only three examples are given, and they are, in fact, heterogeneous:

Ubykh *wa(n)q̇ə̄* 'goat' : PC **waq:ə̄jə̄* 'interjection for driving away calf or cow' is rather dubious semantically, but even if true, fits well the correspondence formulated above (Abkh. *q̇ : Ub. q̇ : PC *q:);
Ubykh *q̇Iə̄-*, Abkhaz **cə̄-ħa-*, PC **c:a-q:a-* 'to bite' is in fact a different correspondence (with -ħ- rather than *ʕ in Abkhaz), in which I reconstruct, of course, a pharyngealized *q̇I (see below). In an expressive reduplicated

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version of this root we have Ub. *q̇Iaq̇Iə̄-,* but Abkh. - with loss of pharyngealization - **q̇aq̇ə̄-* 'to gnaw, crush by teeth'.

The only example of Abkh. *ʕ corresponding to PC *q:- is Abkh. **ʕa-*, PC **q:a-* 'hither' - which is a good semantic and phonetic match, for which I reconstruct PWC **q:Ia-.*

We see that the correspondence, in fact, falls apart, but instead we can observe a promising connection of Ubykh pharyngealization with Abkhaz emphatic laryngeals (ħ, ʕ), see more on that below.

b) Abkh. *q̇ : Ub. q̇, q̇I : PC *ʡ (Kuipers and Chirikba denote it as *q̇) < PWC *q̇.

Above (p....) I have already discussed Chirikba's refusal to reconstruct distinct phonemes *q̇ and *ʡ for Common Abkhaz. Here, I believe, it is exactly this refusal that has served him a bad favour.

If we analyse the examples present on pp. 217-218 we shall easily see that most of the cases presented as the correspondence "PC *ʡ : Abkh. *q̇" are actually cases of the correspondence "PC *ʡ : Abkh. *ʡ". Cf.: PC **ʡa* (Chirikba: **q̇a-*) : Ub. *q̇a-* : Abkh. **ʡa-* 'to be, exist' (see above); PC **ʡə̄* 'locative preverb' : Ub. *q̇a* 'place' : Abkh. **-ʡa* 'directional suffix' (see above); PC **g^{W}ə̄-ʡa-* 'to worry, be anxious' : Ub. *(ǵə̄) q̇ə̄-* 'love, like': Abkh. **g^{W}a-ʡə̄-* 'to worry, care' (Abkh. *-g^{W}a-q̇-,* Ashkh. *g^{W}a-q̇-,* Tap., with assimilative labialization, **g^{W}a-ʡ- > g^{W}aʕ^{W}-*).

For the correspondence Abkh. *ʡ : Ub. q̇ I can also add: Ubykh *š̆^{W}əq̇á* : Abkh. **š̆^{W}ə̄ʡV* 'letter, document' (see NCED 976).

Here, therefore, we have a clearly defined correspondence:

Abkh. *ʡ : Ub. q̇ : PC *ʡ

The presence of an emphatic laryngeal in Abkhaz here again suggests original pharyngealization - this time, lost in Ubykh, but having preserved a trace (phonetically both Abkh. (preserved today in Tapanta) ʡ and Circassian ʡ are certainly pharyngealized). The exact reconstruction here is somewhat complicated: since Ubykh normally preserves pharyngealization, there arises a question - why was it lost in this case?

From synchronic descriptions of languages possessing pharyngealization (such as Rutul, Tabasaran, Archi etc.) we know that pharyngealized and nonpharyngealized uvulars tend to be neutralized in a position of palatalization, usually, before front vowels. In some dialects the resulting forms are pharyngealized, in others they lose pharyngealization. My hypothesis, therefore, was that the factor that could have brought about the loss of pharyngealization in Ubykh, could have been palatalization.

In this way I come to the solution presented in NCED - i. e. to reconstructing a set of PWC palatalized pharyngealized consonants. In the case being discussed one could reconstruct something like PWC *q̇́I. There is, however, also a small set of examples where PC *ʔ and Ubykh q̇ correspond to Abkh. *ʕ (more rarely, *ħ):

PC **ʔa-ç̣^{W}V* 'sweet' : Ub. *q̇a-q̇ə̄ :* Abkh. **qa-ʕa* id.; PC **ʔa-* (Kab. *žə̄-ʔa-*)'speak', **g^{W}ə̄č̣ə̄-q̇a* 'word, speech' : Ub. *q̇a* 'speech, to speak'. Chirikba (p. 217) lists Abkh. **q̇a-ʕa* 'to shout, speak very loudly', comparing the first part with PC and Ub. - but in fact it is the second part that should be compared: cf. without the element *q̇a*-Tap. *aʕa-* 'to hear', as well as the voiceless variant **ħa* 'to hear, reach (of sound)' (Abkh. *-ħa-,* Tap. *ħa-, ʕa-ħa-*).

In NCED I have reconstructed *q̇́I for the latter set of correspondences, which left the only possibility for the row Abkh. *ʔ : Ub. q̇ : PC *ʔ, namely, PWC *q́:I. As a matter of fact, I am not quite sure where to reconstruct *q́:I, and where - *q̇́I (and the external evidence does not help very much, either - although the parallel for **q̇́Ia* 'sweet' - EC **q̇ĕħlV* 'bitter' - suggests rather *q̇́I than *q́:I), but I am reasonably sure that we must reconstruct palatalized pharyngealized uvular affricates for both rows.

We see, therefore, that the beautiful correspondence *q̇ : q̇ : *q̇ proposed by Chirikba does not in fact exist. Besides all the examples already discussed, the list contains:

PC **ʔa,* Ub *q̇a-ṗa* 'hand, arm' : Abkh. **q̇aç̣a-* 'do, make' - an example dubious semantically;
PC **-ʔa* in **psə̄-ʔa* 'wet', **č̣ə̄-ʔa* 'cold' (Chirikba also adds **g^{W}ə̄-ʔa*'to worry, care' which has nothing to do with the preceding two words, see above), Ub. *-q̇ə̄* in *q̇a-q̇ə̄* 'sweet' (on *q̇a-* see above), *ǯə̄-q̇-* 'to salt' : Abkh. **q̇ə̄* 'to ram, press, squeeze' (??)

In both cases the PC and Ubykh forms fit each other very well, but the Abkhaz matches are very dubious.

Palatalized uvulars

1. PWC *q́ > PC ?, Ub. q́, Abkh. χ́

This correspondence, observed in one item (Ub. *-q́a* 'tip, little part' : Abkh. **-χ́a* id.), seems probable, and I am quite willing to add it to my system of correspondences (in NCED the phoneme *q́ is not reconstructed). The other example given by Chirikba, however, seems to me more dubious: I would rather compare Ub. *q́a* 'horn' not with Abkh. *-χ́a* in *á-č^{V}-χ́a* 'horn for wine' (where *-χ́a* is most probably the same as the preceding root), but with Abkh. **-ʕ^{W}a* in **ṭ^{W}ə̄-ʕ^{W}a* 'horn', PC **-q:^{W}a* in **bźa-q:^{W}a* 'horn', reconstructing PWC **q́:^{W}a* (see NCED 903).

The third and last example, given by Chirikba, demonstrates actually a different correspondence: Ub. q́ : Abkh. *q (Ub. *mǝ̄q́á* 'bosom' : Abkh. **maqa* 'shoulder with forearm), for which I reconstruct PWC *q́:, because it is perfectly parallel to the correspondence Ub. q : Abkh. *q < PWC *q: (see above); unfortunately, the root has no Circassian matches. Chirikba, with his unwillingness to reconstruct tense consonants in PWC, is once again forced to confuse two different correspondences.

2. PWC *G > PC *ɓ, Ub. ɓ́, Abkh. ɓ́

This correspondence is present in Chirikba's book (pp. 283-284); moreover, the author has found the Circassian reflex (*ɓ), absent in my chart (NCED 183), and observable in PC **ɓ́ačǝ̄* 'milk products' (: Ub. *čaɓ́a,* Abkh. **čaɓ́a*) and PC **žǝ̄-bɓ́a* 'strong wind' (: Abkh. **bɓ́ǝ̄-ʕa* 'swift; brave'). However, here, too (as in the case with *G) Chirikba reconstructs *ɓ́ (in his notation - *ɣ́), because in the row "PC *ɓ : Ub. ɓ́ : Abkh. *ʕ" he reconstructs a palatalized *ʕ́ (p. 291).

I have already written above about the implausibility of the change *ʕ > ɓ; the change *ʕ́ > ɓ́ is no less implausible - in fact, implausible is the very supposition of the distinction *ʕ - *ʕ́ (as well as *ħ - *ħ́, see below). I am not aware of a single language with the opposition of palatalized - non-palatalized emphatic laryngeals (pharyngeals). So here again I prefer to reconstruct:

PWC *ɓ́ > PC *ɓ, Ub. ɓ́, Abkh. *ʕ
PWC *Ǵ > PC *ɓ, Ub. ɓ́, Abkh. *ɓ́

As for the phoneme *Ǵ, reconstructed by Chirikba (p. 209), it is based only on one rather dubious example (PC **-q:a* 'affirmative suffix' : Abkh. **ç̣^{w}a-ɓ́a* 'precisely, exactly') and the author himself states that the reconstruction "is not based on reliable material and thus remains tentative".

3. PWC *q́ > PC *ʡ (Chirikba's notation - *q̇), Ub. q́, Abkh. *q́

This is the same correspondence (for once!) as in Chirikba 219. A minor difference consists in Chirikba's adding a parallel reflex q̇I in Ubykh (due to the authors conviction in the secondary nature of Ubykh pharyngealization), which is present only in one example (Ub. *q̇IaLa* 'turbulent, noisy (as child or animal) : Abkh. **q̇ala* 'vagrant, stray, tramp'; I think that both Ub. and Abkh. are actually borrowed from PC **ʡa-Lǝ̄* 'wild', the opposite of **ʡa-sa* 'tame'), and can be safely dismissed.

Labialized uvulars

1. PWC *q^{w} > PC *q^{w}-,q:^{w}, Ub. q^{w}, -χ^{w}- , Abkh. *ħ^{w}

This correspondence is observed in:

PC _*tħarə̄q:^{w}a_ : Ub. _daχ^{w}á_ : Abkh. _*laħ^{w}a_ 'rook, raven' (considered by Chirikba 216 to be irregular)
PC _*ħa-q^{w}a_ 'trough', _*q^{w}a-s^{w}a_ 'boat' : Abkh. _*jaħ^{w}a_ 'trough' (this parallel is placed by Chirikba into his PWC *G^{w}, see p. 209, although it does not fit into his system, see below);
Ub. _q^{w}ə̄nə̄_ 'mouse' : Abkh. _*ħ^{w}ə̄na-pə̄_ id.
Ub. _pqə̄_ 'fruit' (with dissimilation < _*pq^{w}ə_) : Abkh. _*pə̄ħ^{w}a_ 'plum' (see NCED 873; the PC parallel here has an irregular fricative - PC _*pχa-_ / _*pχ^{w}ə̄-_ 'fruit', probably due to the position in the consonant cluster).

Despite some scarcity of evidence (PWC *q^{w} is a rather rare phoneme, just like *q), the general behaviour of the affricate is here the same: Abkhaz demonstrates a spirantization (*q^{w} > ħ^{w}), while Ubykh and Circassian preserve (with some positional exceptions) the affricate. The tenseness of *q:^{w} in PC here is not surprising: in PC *q and *q^{w} occur almost exclusively in the initial position (Kuipers 1975 lists some words with intervocal *q, *q^{w}, but none of them has any external parallels), while *q: and *q:^{w} are abundant in all positions; we may think that PC had a neutralization of medial *q(^{w}) and *q:(^{w}).

In Chirikba 213 we observe a correspondence with three unexplainable "commas": PWC *q^{w} > PC *q^{w}, *q:^{w}, Ub. q^{w}, qI^{w}, qI, Abkh. *q^{w}. Let us look at the material closer.

The vast majority of examples actually pertain to the phoneme that was reconstructed in NCED as *q:^{w} and gives the following reflexes: PC *q:^{w}, Ub. q^{w}, Abkh. *q^{w} (cf. PWC *q: > PC *q:, Ub. q, Abkh. *q, see above). These are examples (2) (PC _*λa-q:^{w}a_ 'leg, foot' : Abkh. _*šə̄-q^{w}a_ 'heel'), (3) (PC _*q:^{w}ə_ 'thrash, beat' : Abkh. _*q^{w}a-q^{w}a-_ id.), (4) (PC _*q:^{w}ə̄_ 'fill up, compress' : Abkh. _*q^{w}a_ 'to press, felt'), (5) (PC _*q:^{w}a_ 'prop up, support' : Abkh. _*q^{w}a-_ 'to help, support'), (7) (PC _*q:^{w}a_ 'body' /in compounds/ : Abkh. _*q^{w}a-_ id.), (8) (PC _*pχa-q:^{w}a_ 'torch' : Ub. _mʒa-q^{w}a_ 'moonlight' : Abkh. _*s^{w}a-q^{w}a_ 'ray, beam'), (9) (PC _*q^{w}ama_ : Abkh. _*maq^{w}ə̄_ 'twig'), (10) (PC _*q^{w}a_ : Ub. _q^{w}a_ 'son'), (11) (PC _*q:^{w}a_ : Ub. _q^{w}a_ 'valley'), (12) (PC _*šə̄q:^{w}ə̄_ 'vessels' : Abkh. _*ašə̄q^{w}a_ 'boat').

Circassian *q^{w} is present here only in one rather dubious example: PC _*q^{w}a-ja_ 'cheese' : Abkh. _*q^{w}arə̄ta_ 'basket for keeping cheese'. The Abkh. word actually means "basket for keeping food" and is phonetically and semantically closest to PC _*pχ^{w}anta_ 'box, basket' (see the discussion in Shagirov 2, 25); here *pχ^{w}- in PC could be a secondary development from *pq:^{w}- (just as in *pχa-, *pχ^{w}ə̄- 'fruit', see above), thus allowing to reconstruct PWC _*pq:^{w}arə̄ta_ ~ _*pq:^{w}anə̄ta._
Ub. pharyngealized qI(^{w}) is present in two examples: Ub. _qI^{w}ə̄-_ 'to bark' : PC _*ħa-q:^{w}ə̄-_ id., _*q:^{w}ə̄-ƃə̄-_ 'to howl' : Abkh. _*q^{w}a-ʕə̄_ to howl' (= PC _*q:^{w}ə̄-ƃə̄-_). Ub.

qIʷə̄- is used usually together with *wIa* 'dog', and here indeed pharyngealization may be secondary.
Ub. *qIaLa* 'jackdaw' : PC **q:ʷaLa* id.
Cf. also Ub. *qIa-śə̄* 'village' : PC **q:ʷa-ǯa* id. In the last two examples there are no Abkhaz matches, but there exists at least one example where Ubykh qI corresponds to Abkhaz *ʕʷ: Ub. *qIa-* 'to run' : Abkh. **ʕʷə̄-* id.

We can, therefore, rather safely reconstruct: PWC *q:Iʷ > PC *q:ʷ : Ub. qI (note the peculiar absence of labialisation) : Abkh. *ʕʷ (for *q:I > PC *q:, Abkh. *ʕ see above).

Other good examples of this correspondence are, therefore:
PC **tq:ʷa* 'crocodile' : Abkh. **t̯əʕʷa* 'monster eating moon or sun'
PC **p:q:ə̄* 'carcass, skeleton' (< **p:q:ʷə̄* by dissimilation) : Abkh. **bVʕʷə̄* 'bone'.

The latter two examples are for some reason considered by Chirikba to reflect his PWC *Gʷ - which otherwise gives quite different reflexes (PC *ʡʷ, Ub. q̇ʷ, Abkh. *ʕʷ) and corresponds to my *q́:Iʷ.

2. PWC *Gʷ > PC *ɓʷ, Ub. ɓʷ, Abkh. *ɣʷ

The story here is quite similar to *G and *G. Chirikba acknowledges the correspondence, but reconstructs here *ɓʷ (p. 284), while instead of my *ɓʷ (for the correspondence PC *ɓʷ : Ub. ɓʷ : Abkh. *ʕʷ) he reconstructs *ʕʷ (p. 291-293). I shall not repeat my argumentation here.

3. PWC *q̇ʷ > PC *q:ʷ, Ub. q̇ʷ, Abkh. *q̇ʷ.

This correspondence (completely parallel to *q̇, see above) is observed in:

PC **q:ʷə̄rtə̄* 'brood-hen' : Ub. *q̇ʷart* id. : Abkh. **q̇ʷarə̄tə̄* id.
PC **q:ʷa-(n)č̣a* 'crooked' : Ub. *q̇ʷa-(rta)-* 'bend' : Abkh. **q̇ʷə̄-* id.
PC **q:ʷə̄- / *q:ʷa-* 'branch, shoot' : Ub. *ra-q̇ʷá* id. : Tap. *q̇ʷə̄-ra* 'dry grass'
Ub. *q̇ʷač̣a-* 'rumple, rub, crumple' : Abkh. **q̇ʷə̄č̣ə̄-* id.
Ub. *c̣aq̇ʷ* 'manure' : Tapanta *q̇ʷə̄c̣* id.
Ub. *čaq̇ʷə̄* 'basin' (faultily recorded with pharyngealisation by Dumézil; Vogt's recording shows no trace of it): Abkh. **čaq̇ʷə̄* 'big wooden mug'.

Chirikba rejects this correspondence and presumes the Circassian correspondence for Ub. q̇ʷ, Abkh. q̇ʷ to be *ʡʷ (*q̇ʷ in his notation). Let me try a critical analysis of his lexical evidence:

(2) Abkhaz **q̇ʷaq̇ʷa* 'cut, cleave' hardly corresponds to PC **ʡʷa* 'thresh', Ub. *q̇ʷa* 'trample down, press'; a much better match is Abkhaz **ʕʷə̄* 'to shake, beat up' (on the correspondence PC *ʡʷ : Ub. q̇ʷ : Abkh. *ʕʷ see below);

(3) *-ʔʷə̄* in PC **ç̣ʷa-ʔʷə̄* 'sour' does not correspond to Abkh. **q̇ʷə̄-ja* 'to steam body or its parts' etc.: the PC form is a good match for Ub. *ʒʷaq̇ə̄* and Tap. *č̣ʷq́a* 'sour' (where the second consonant corresponds rather irregularly: it probably goes back to *q̇(ʷ) with various assimilations and dissimilations, see the discussion in NCED 521);

(4) PC **ʔʷa-śħa* 'barrow, hill' corresponds to Abkh. **q̇ʷa-ra* 'coast, shore' (cf. NCED 939), but Ub. *šə̄-q̇ʷa* 'upwards' does not belong here (being a good match for PC **-ʔʷə̄* 'upper part' - for some obscure reason compared in No 8 with Abkh. **q̇ʷə̄-* 'bend', see above)

(5) The match between PC **ʔʷa* 'pen, fold', Ub. *ǵə̄-q̇ʷa-* 'to close' and Bzyp *a-ʒajχa-q̇ʷa* 'spring fenced by stones' seems very dubious phonetically and morphologically (the latter form belongs perhaps rather to the previous etymology - or else, quite independently, should be compared with Ub. *q̇ʷa-da* 'stone trough' - for some unclear reason compared by Chirikba in No 11 with Abkh. **q̇ʷada* 'coagulated blood').

(7) Ub. *q̇Iʷa* 'cave, ravine' should not be compared with Abkh. **-q̇ʷa* bow' (in **cʷa-q̇ʷa* 'rainbow'); it corresponds rather well to PC **q:ʷa* 'ravine' (which itself was borrowed into Ub. *qʷa* id.)

On the whole, if we eliminate dubious comparisons, we are left with the following material:
1.PC **ʔʷə̄* 'mouth' : Abkh. **q̇ʷə̄-* 'preverb (*from the mouth)'
2. PC **ʔʷa* 'thresh' : Ub. *q̇ʷa-* 'trample down, press' : Abkh. **ʕʷə̄* 'to shake, beat up'
3. PC **ʔʷa-* 'barrow, hill' : Abkh. **q̇ʷa-ra* 'coast, shore'
4. PC **-ʔʷə̄* 'upper part' : Ub. *šə̄-q̇ʷa* 'upwards'
5. Ub. *q̇Iʷa* 'cave, ravine' : PC **q:ʷa*
6. Ub. *q̇Iʷə̄q̇Iʷ-*, Abkh. **q̇ʷə̄q̇ʷə̄-* 'to huddle oneself, to squat'

Now the picture becomes somewhat clearer. Ubykh q̇Iʷ certainly corresponds to PC *q:ʷ (not *ʔʷ!). The Abkhaz correspondence here is less clear. We have *q̇ʷ in **q̇ʷə̄q̇ʷə̄-* 'to huddle oneself, squat', but this is an expressive reduplication with possible irregularities. On the other hand, there are two other etymologies (only the first of them is discussed by Chirikba, p. 216, but considered to be an "irregular correspondence"):

Ub. *q̇Iʷə̄* 'hair' : PC **q:ʷə̄-jə̄* 'bald, hairless' (-jə̄ is a privative / pejorative suffix) : Abkh. **qʷə̄* 'hair';
Ub. *q̇Iʷə̄-* 'bend' : Abkh. **qʷa-* 'bend' (note that *q̇Iʷə̄q̇Iʷ-* can actually represent the same root; in Abkhaz, besides **q̇ʷə̄q̇ʷə̄-*, we also have **qʷaqʷa* 'crooked').

Thus, here again we have a special row of correspondences where Ubykh has pharyngealization, which allows us to reconstruct safely PWC *q̇Iʷ > Ub. q̇Iʷ, PC *q:ʷ, Abkh. *qʷ (with q̇ʷ as an expressive variant).

In the above examples PC *ʡʷ corresponds to Ub. q̇ʷ and (once) to Abkh. *ʕʷ. Other examples can be added, notably:

PC **ṭʡʷə̄* 'two' : Ub. *ṭq̇ʷa* : Abkh. **ʕʷə̄* id.;
PC **da-ʡʷa-* 'hear' : Ub. *La-q̇ʷə̄-* : Abkh. **ʒ́ə̄-rə̄-ʕʷə̄-* id.

On the analogy with *q́:I (PC *ʡ : Ub. q̇ : Abkh. *ʡ, see above) we can safely reconstruct *q́:Iʷ in all these cases. Chirikba (p. 209) acknowledges this correspondence, but reconstructs here a voiced *Gʷ (cf. the discussion of his non-labialized *G above). However, he adds three absolutely different examples that allow him to split the Circassian reflex into three - not just *ʡʷ, but also *qʷ and *q:ʷ - without any motivation! The two examples with *q:ʷ are PC **tq:ʷa* 'crocodile' - Abkh. **ṭə̄ʕʷa* 'monster eating moon or sun' and PC **p:q:ə̄* (< **p:q:ʷə̄*) 'carcass, skeleton' - Abkh. **bVʕʷə̄* 'bone' that were discussed above and are in my system reconstructed with *q:Iʷ; the one example with *qʷ is PC **ħaqʷa* 'water-trough for cattle' - Abkh. **jaħʷa* 'trough', which, as I have shown above, is a quite regular case of PWC *qʷ.

Glottalized *q̇́Iʷ is more difficult to reconstruct, but it may be supposed in:

PC **ʡʷə̄-* 'be heard' : Ub. *q̇ʷə̄-* id. : Abkh. **ħa-* id. (with secondary delabialisation) - although here it may have been *q̇́I, with secondary labialisation in Ub. and Adygh, under the influence of **q̇́I:ʷə̄-* 'hear';
PC **ʡʷa-tə̄-* 'crumple' : Abkh. **ħʷa-* 'to tan (leather)'.

As for the few words where Abkhaz has *q̇ʷ corresponding to PC *ʡʷ (but never at the same time to Ubykh q̇ʷ!) - they actually fall into quite a different row of correspondences, where Ubykh has q́, see below.

Labialized palatalized uvulars

All these phonemes are rare, but have very systematic reflexes.

1. PWC *q́ʷ > PC *qʷ-, Ub. χ́, Abkh. *ħ(ʷ)

Interestingly enough, Chirikba accepts this correspondence (present in the PWC word for 'pear', **q́ʷa* > PC **qʷə̄-ź ə̄*, Ub. *χ́a*, Abkh. **ħa*) and also reconstructs *q́ʷ here. It is, however, easy to see that the reconstruction *q́ʷ is quite symmetrical to *q and *qʷ in my system (cf. the regular fricativization in *q > Abkh. *ħ, *qʷ > Abkh. *ħʷ, but absence of fricativization in Chirikba's system, where *q > Abkh. *q and *qʷ > Abkh. *qʷ).

2. PWC *q́:I^w > PC *q:^{w}, Ub. q́, Abkh. *ʕ^w

This correspondence is attested in the word for ’horn’, see above; since Chirikba does not accept tense consonants in PWC, he treats the material in a different way, which is less convincing to me.

3. PWC *G^w > PC *ɓ^w, Ub. ɓ́, Abkh. *ɓ^w

This phoneme can be reconstructed in: PC _*s^{w}ə̄-ɓ^{w}a-_ ’to hate, envy’ : Ub. _c^{w}ə̄-mɓ́ə̄-_ id. : Abkh. _*c^{w}ə̄-mə̄ɓə̄-_ id. (with -ɓ- probably dissimilated < _*c^{w}ə̄-mə̄ɓ^{w}ə̄-_).

On analogy with other voiced uvulars (*G, *G^w) one would expect Chirikba to reconstruct *ɓ́^{w} here. However, he seems not to be aware of this example and reconstructs *ɓ́^{w} for the numeral ’nine’ (PC _*bɓ^{w}ə̄,_ Ub. _*bɓ́ə̄,_ Abkh. _*z^{w}ə_) - where I, with some doubt, reconstruct velar *ɣ́^{w} (see NCED 183). As for the phoneme for which I reconstruct *ɓ́^{w} (PC *ɓ^w : Ub. ɓ́ : Abkh. *ʕ^w), Chirikba again reconstructs a quite improbable palatalized labialized emphatic laryngeal (pharyngeal) *ʕ^w, see p. 296, thus repeating the same that he did with PWC *ɓ, *ɓ́ and *ɓ^w, see above.

4. PWC *q̇́^{w} > PC *ʡ^w, Ub. q́, Abkh. q̇^{w} / ʕ^w

This correspondence is more or less the same as in Chirikba 222. I must, however, make two remarks here:

a) Abkh. _*q̇^{w}ə̄na_ ’decoration on belt; belt, girdle’ and Ub. _q̇Iə̄na_ ’clasp’ can not be regarded as cognate to PC _*ʡ^{w}ə̄na_ ’nail’; they are most probably borrowed from PC _*ʡ^{w}ə̄na_ ’clasp’, derived from _*ʡ^{w}ə̄-_ ’to clasp, buckle’ (nowadays mainly with preverbs: cf. Kab. _ṣ́ə̄-ʡ^{w}ə̄-_ ’to clasp’, _ṣ́ə̄-ʡ^{w}ə̄-na_ ’clasp, buckle’); the latter corresponds well to the Ubykh verbal root _q̇́ə̄-_ ’to clasp’ and to Abkh. _*qa-ʕ^{w}a-_ ’to shut up’see Chirikba, p. 210 (this is the only root where he reconstructs voiced *G^w). Therefore I would still stick to my etymology presented in NCED 527, where PC _*q̇^{w}ə̄na_ ’nail’ is compared with Ub. _nə̄q̇́ə̄_ id. and Abkh. _*c̣^{w}ə̄-ʕ^{w}anə̄_ ’stake, peg’ (Chirikba would have reconstructed here *G^w, too).

Thus, the Abkhaz reflex here appears to be *ʕ^w. The Circassian and Abkhaz forms, therefore, reflect a secondary pharyngealization here (cf. the development of *q́:I^w and *q̇́I^w described above), which is not surprising for palatalized uvulars (cf. also the development *q́ > *ʡ in Circassian, see above).

b) The correspondence PC *ʡ^w : Abkh. *q̇^{w}, observed in a few cases, probably goes back to the same PWC phoneme, with a variation q̇^{w} / ʕ^w in Abkhaz.

Pharyngealized uvulars.

I have shown above that despite Chirikba’s skepticism, Ubykh pharyngealized uvulars in fact belong to quite distinct rows of correspondences. Here I shall just sum up my reconstruction of pharyngealized uvular stops (affricates):

PWC PC Ub. Abkh.
(*qI)

*q:I *q: *ʕ
Cf. _*q:Ia-_ 'locative preverb' (see above)

*q̇I *q: q̇I *ħ
Cf. _*q̇Iə̄-_ 'bite' (see above)

*q́I *ħ q *q
This correspondence (ignored by Chirikba) is observed in:
PC _*c:ə̄-ħV-_ 'trust' : Ub. _qā-c̣a-_ : Abkh. _*qa-c̣a-_ id.
PC _*ħaLV_ 'wedge' : Ub. _qaLa_ id.
PC _*śħa_ 'head' : Abkh. _*qa_ id. (the Ubykh form _ša_ here reflects a contraction of the cluster *Sq́I-)

Here, as also in the case of other palatalized pharyngealized uvulars, Ubykh loses pharyngealization while Circassian preserves its trace in the emphatic laryngeal (pharyngeal) *ħ.

*q́:I *ʡ q̇ *ʡ
Cf. _*q́:Ia-_ 'to be', _*q́:IV_ 'place, locative affix', _*g^{w}ə̄-q́:Ia_ 'to worry' (see above)

*q̇́I *ʡ q̇ *ʕ / *ħ
Cf. _*q̇́Ia_ 'sweet', _*q̇́Ia_ 'speak' (see above)

*qI^{w} *q^{w}- χI^{w} *ħ^{w}
This correspondence (regarded as "irregular" by Chirikba on p. 216, but perfectly well explained if one assumes pharyngealization in PWC) is observed in: PC _*q^{w}a_ : Ub. _χI^{w}a_ : Abkh. _*ħ^{w}a_ 'pig, swine'

*q:I^{w} *q:^{w} qI *ʕ^{w}
Cf. _*q:I^{w}aL'a_ 'jackdaw', _*q:I^{w}a-_ 'village', _*q:I^{w}V-_ 'to run', _*ṭə̄q:I^{w}a_ 'monster', _*bə̄q:I^{w}ə̄_ 'bone, skeleton' (see above)

*q̇I^{w} *q:^{w} q̇I^{w} *q̇^{w} / *q^{w}
Cf. _*q̇I^{w}a_ 'ravine', _*q̇I^{w}ə̄-_ 'bend' (and _*q̇I^{w}ə̄q̇I^{w}ə̄-_ 'to huddle oneself, squat'), _*q̇I^{w}ə̄_ 'hair' (see above)

*q́I^{w} *ħ χ^{w} *q^{w}
This correspondence (ignored by Chirikba) is very similar to *q́I (except for

obvious labialization); note that Ubykh has χ^{w} - fricativized (as in *qI^{w} > χI^{w}) and depharyngealized (as in all other palatalized pharyngealized rows). It is observed in:

PC **ħa* 'wheat' : Ub. *χ^{w}a* : Abkh. **q^{w}ə̄-ʒə̄* id.;
PC **ħa-dá* 'dead body' : Tapanta *q^{w}a-di* id.
PC **ʔa-ħa* 'part' : Abkh. **q^{w}ə̄* id.

*q́:I^{w} *ʔ^{w} q̇^{w} *ʕ^{w}
Cf. **q́:I^{w}a-* 'thresh', **Tq́:I^{w}V* 'two', **q́:I^{w}V-* 'hear' (see above)

*q́I^{w} *ʔ^{w} q̇^{w} *ħ^{w}
Cf. **q́:I^{w}V-* 'be heard, resound', **q́:I^{w}a-* 'crumple, tan' (see above).

It is easy to see that the correspondences for pharyngealized uvulars form a rather elegant system (note also its interesting feature, distinguishing it from the system of other uvulars - a complete lack of voiced pharyngealized stops / affricates) and allow to explain rather many cases treated by Chirikba as unmotivated splits or just "irregular".

Front (and middle) sibilant affricates.

A preliminary remark:

Chirikba reconstructs front sibilant affricates and middle sibilant affricates, although the two series are in evident complementary distribution, and, in fact, form a single series: *C - *C - *C^{w} - *C^{w}, just as in other local series of consonants. The second and third member of this quaternion are regarded by Chirikba as "middle sibilant affricates", and he does not reconstruct the palatalized labialized affricates at all.

I must say that the part of the book concerning sibilants is probably the weakest. Chirikba here completely ignores the real complicated situation with the affricate correspondences and devises an almost imaginary system which needs to be totally rewritten. He completely disregards the system presented in NCED and basically projects the Common Abkhaz system of affricates into the WC reconstruction - which is, from my point of view, absolutely untenable. Below I shall try to give a systematic discussion of all the intricacies of PWC sibilant reconstruction (what I tried to do in NCED, pp. 185-187 - briefly and apparently unsuccessfully, judging from a complete disregard of all my arguments in Chirikba's book).

Let us discuss, one by one, all correspondences proposed by Chirikba.

1. WC *ʒ > PC *ʒ : Ub. ʒ : Abkh. *ʒ

Except for the fact that PC has here a variation *ʒ / *z (the fact mentioned in NCED and implicitly present in Chirikba's examples) this seems to be the same correspondence as in NCED (p. 181). Minor corrections:

a) Common Abkhaz has not **mə̄ʒa,* but **mə̄za* 'moon, month', and for PC **maza* : Ub. *mə̄ʒa* : Abkh. **mə̄za* I reconstruct not WC **mə̄ʒa,* but **mə̄za.*

b) instead of **mə̄ʒa* 'light' I reconstruct **bə̄̃za,* adding here also PC **bzə̄-jə̄* 'ray'.

c) Chirikba is stretching the evidence while comparing Ub. *mə̄ʒa-ɓʷanə̄* 'spruce, silver fir' with PC **mazə̄* 'forest' and Abkh. **mə̄za* 'pine'. It does not really matter if *mə̄ʒa-ɓʷanə̄* is a misrecording of Mészáros's (as suggested by Dumézil) or a compound 'light + tree' (as suggested by Mészáros himself); in any case the comparison of the Abkh. and Adyghe forms with Ubykh *mə̄ʒ́ə̄* 'prickle' (otherwise left without etymology) seems to me much more preferable.

Chirikba refutes it because of "the irregular correspondence between sibilants" (p. 225) - which is quite wrong. We have very similar correspondences in: PC **wə̄-zə̄* 'illness' (Chirikba p. 255: "etymology remains unclear"(?) : Ub. *źa-ɓʷá* id. : Abkh. **č̓ə̄ma-za-* 'be ill' (the word means just 'to be ill' in all dialects and I can not agree with Chirikba's treating it as 'have a tumour', see p. 255); PC **q:a-bzá* 'clean' : Ub. *ṗć̣a* (assimilation < **ṗʒ́a*) id. : Abkh. **bə̄zə̄-ja* 'good'.

In this case Chirikba obviously tries to dismiss several well established lexical parallels just because he does not want to violate his system of correspondences, according to which "middle" affricates and sibilants should correspond to each other in all WC branches. See the discussion of *ź, *ś and *ʒ́ (as well as *ž, *š and *ǯ) below.

2. WC *c > PC *c:, *c : Ub. c : Abkh. c

Here we very obviously deal with two correspondences:

a) WC *c > PC *s-, c : Ub. c : Abkh. c

PC *s and *c are here in complementary distribution (*s appearing word-initially, *c - word-medially; note that word-initial *c- is extremely rare in PC, being actually present only in one root - **cə̄* 'hair', having no known etymology), cf.:
PC **sa-* 'get accustomed' : Ub. *ca-* : Abkh. **š̓ə̄-ca-* (Chirikba lists this example, but does not mention PC *s in the header of his correspondence, evidently, trying to avoid an unpleasant unmotivated three-way split of WC *c into PC *c:, *c and *s);
PC **sə̄-* 'to burn' : Ub. *ca* 'hot' : Abkh. **ca* id. (Chirikba compares here PC **c:asa* 'spit' which actually does not exist: Kab. *ʒāsa* is a secondary variant (possibly under influence of sa 'knife'), cf. Ad. *cāca* < **c:ac:a;* see the discussion in NCED 1091. Anyway, **sə̄-* 'burn' seems to be a much better match for 'hot' than 'spit').
PC **warə̄ca* 'dung' : Abkh. **wacə̄.*
PC **λa-χ-cə̄* 'offshoot from the root' : Abkh. **-cə̄* 'root'.

b) WC *c: > PC *c: : Ub. c : Abkh. c Most other roots (**c:a* 'tooth', **c:V* 'grain', **c:ə̄GwV* 'mouse') belong to this category.

Here, as in all other cases, Chirikba has to suppose an unmotivated split in Circassian in order to lump together the reflexes of *c and *c:.

3. WC *ç̣ > PC *ç̣ : Ub. ç̣ : Abkh. *ç̣

This correspondence is the same as in NCED (p. 181).

4. WC *ʒ́ > PC *ʒ, *ź : Ub. ʒ́ : Abkh. *ʒ́

The reflexes of *ʒ́ in NCED (p. 181) are formulated as: PC *ʒ / z, Ub. *ʒ́, Abkh. *ʒ́. Chirikba includes into his examples forms with PC *z (**za* 'cornel fruit', **zə̄* 'decant, filter'), thus basically agreeing with the correspondence formulated in NCED, although for some reason he does not include PC *z into his tables of correspondences.

However, he lumps together also forms with PC *ź, such as PC **źa* : Abkh. **ʒ́ə̄* 'to roast, bake' and PC **źə̄-jə̄* 'young of animals' : Abkh. **ʒ́ə̄-śə̄* 'kid'. These actually belong to a quite different set of correspondences, where Ubykh has ž^{w} and where I (NCED 182) reconstruct PWC *ź^{w}:

PC **źa* : Ub. *ž^{w}a-* : Abkh. **ʒ́ə̄-* 'to roast, bake'

PC **bźa* 'horn; hoof, (finger/toe)nail' : Ub. *-ž^{w}a* '(finger/toe)nail' : Abkh. **-mə̄źa* in Tapanta *š́a-mza* 'hoof'.

PC **źaźa* 'slow' : Abkh. **źaźa* id.

The reason why Chirikba does not accept these parallels is his preconception that "there are no traces of labialised-palatalised affricates" (p. 223) - which, as I intend to show, is quite wrong - see below.

Also, quite unexpectedly, in No 10 we meet Ub. *za-* 'to decant, filter' corresponding to PC **zə̄* and Abkh. **r-aʒ́a-* id. This is, in fact, one of the rather numerous set of correspondences, completely ignored or mistreated by Chirikba, where Abkhaz and Circassian have front affricates while Ubykh has palatalized ("middle") affricates (Circassian preserves frontness or "middleness" only in the glottalized row, probably because of an early variation *ḉ̣ > *ṣ́; note that *ḉ̣ and *ṣ́ are not opposed in PC, which is why I write *ḉ̣, while Chirikba, following Kuipers, writes *ṣ́).

The examples are:

PC **sa* 'yesterday' (in compounds) : Abkh. **ćə̄* id. (in compounds) : Ubykh *ca, wa-cá* 'tonight'[Chirikba, p. 230, omits the Ubykh form and treats the etymology as reflecting PWC *ć - where Ubykh should have ć];

PC **-zə̄* (in **bžə̄-jə̄-zə̄*) 'span' : Abkh. **ʒ́a* id. : Ub. *ʒa* id. [ignored by Chirikba]

PC **zə̄* 'decant, filter' : Abkh. **r-aʒ́a-* id. : Ub. *zə̄-* id. [regarded by Chirikba - contrarily to his own correspondences - as reflecting PWC *ʒ́, see above]

PC **wasa-psə̄* 'dew' : Abkh. **(a)ʒ́V* id. : Ub. *zʷa-ʒə̄* 'snow' [PC **wasa* instead of **waza-* due to contamination with **wasə̄* 'snow' - which actually corresponds to a different Abkh. root, **śə̄-* 'snow', see the analysis in NCED p. 675. Chirikba finds "phonetic differences between all these forms too great to accept their relationship" (p. 224), and ends up in comparing just the Ubykh *ʒə̄-* 'snow' with Tapanta *aʒə̄* 'dew, hoarfrost', simply throwing away all the rest of Abkhaz and Circassian evidence].
Abkh. **ʒ́amə̄ʕʷa* 'cheek' : Ubykh *ʒámɓa* 'chin' [ignored by Chirikba]
PC **ć̣a-* 'to pass (of time)' : Abkh. **ć̣ə̄-* id. : Ub. *c̣a-* id. [Chirikba, p. 233, reconstructs here *ć̣ - which forces him to postulate a tremendous number of unmotivated splits: PWC *ć̣ > PC *ć̣, *c̣; Ubykh ć̣, c̣, ć; Abkhaz *ć̣, *ć - see the discussion of this correspondence below].
Abkh. **rə̄-bə̄ć̣ə̄-* : Ub. *bə̄c̣ə̄-* 'crush, crumple' [same case as the preceding]

There exist also parallel rows of fricative correspondences:

PC **psa-* 'to plane' : Abkh. **pə̄śa-* id. : Ub. *psə̄-* 'to whet' [ignored by Chirikba]
PC **p:c:a-* (assimilation < **p:sa-,* see above) 'fish' : Abkh. **pə̄śə̄-* id. : Ub. *psa* id. [listed by Chirikba in a different section, on p. 337, without any explanation of the correspondence between sibilants]
PC **λa-psa* 'root' : Abkh. **mə̄śa* 'horn, horn matter' : Ub. *λa-msá* 'root' [The Ubykh form is separated from the obvious PC and Abkh. parallels in Chirikba 337, and instead compared with PC **na-p:c:a* 'eyebrow' (??)].
PC **zazə̄* 'gall' : Abkh. **(a)źə̄* id. : (?) Ub. *-ca* in *c̣ʷa-cá* 'gall' (possibly assimilation < **c̣ʷa-zá*). [The Bzyp form recorded in Marr 1926 is *a-ź* and forms a minimal pair with *a-z* 'bush, shrub', thus the reconstruction **zə̄* in Chirikba 249 and his comment on the next page are incorrect.]
PC **za* / **zə̄* 'reciprocity prefix' : Ub. *za-* id. : Abkh. **źə̄* 'for (smb.); prefix of the object version' [ignored by Chirikba]
PC **za-* 'to meet smb.' : Abkh. **źa-* 'to meet, gather' [ignored by Chirikba]

Below I shall show that no rows of correspondences like "PC *č : Ub. č : Abkhaz *č" exist (despite seemingly numerous examples provided by Chirikba). That is why for the peculiar set of correspondences presented above I proposed already in 1978 [Starostin 1978] to reconstruct PWC plain back affricates and fricatives (*č, *ǯ, *č̣, *š, *ž), presuming that they lost the second (back) focus in Ubykh and Circassian (*č > c, *š > s etc.), while the same back focus was transformed to middle focus in Abkhaz (*č > ć, *š > ś etc.). All nonpalatalized back affricates and fricatives in modern WC languages participate in more complicated rows of correspondences (see below) and actually go back only to labialized back affricates: i. e., similar (but slightly different) shifts occurred in all three WC branches, involving first the loss of plain (non-palatalized) back affricates, with a subsequent filling of the freed space by moving either *čʷ or *čʷ to č through delabialisation. This allows

1) to explain all the existing evidence without hiding any of it;
2) to explain the peculiar phonetic development in individual WC branches
3) to explain external evidence (which seems to be the least concern for Chirikba, but is certainly necessary in any historical research). Here I must say that most external (East Caucasian) cognates available for the presented list of cases, have indeed back (hushing) affricates or sibilants: cf. _*č̣VmħV_ 'span' : WC _*ǯa,_ see NCED 391-392; _*Hŏǯāl_ 'strain, milk' : WC _*žV_ 'filter', see NCED 600; _*čămaGŭ_ 'jaw' : WC _*ǯamə̄b́ʷa,_ see NCED 339; _*bɨ ˇn̠šwV_ 'pipe, horn' : WC _*pə̄ša,_ see NCED 307-308; _*ǯăwV_ 'kidney' : WC _*žə̄_ 'gall', see NCED 1106. Exceptions are only _*jămʒĂ_ 'snow' (WC _*ǯV_) and _*bVs̱wA_ 'fish (WC _*p:ə̄šA_) which require some special explanation, but are certainly in the minority.

5. WC *ʒʷ > PC *ʒʷ : Ub. ʒʷ : Abkh. *ʒʷ, *zʷ

In principle, this is a correct correspondence - although I know only one example: PC _*ʒʷa_ 'to dam, fill (with water)' : Abkh. _*ʒʷa-ʒʷa-_ 'to wash' (Chirikba compares the PC form with different Abkh. forms meaning 'vomit' or 'sperm', himself admitting that the comparison is "tentative because of the semantics"). For Ub. _ʒʷa-_ 'drink' : Abkh. _*zʷə̄-_ id. I prefer to reconstruct *zʷ - with the same variation ʒʷ / zʷ in Ubykh as in its reflexes of *z (> Ub. z / ʒ). Thus the Ubykh reflex of *ʒʷ is actually unknown, and all we know of the reflexation of PWC *ʒʷ is > PC *ʒʷ : Abkh. *ʒʷ (see NCED 181).

It is important to note that, although Chirikba treats this as one of the "middle sibilants", from the phonological point of view this is simply a labialized front affricate, and the notation *ʒ́ʷ is plainly excessive.

6. WC *ć > PC *s : Ub. ć : Abkh. *ć

The correspondence is correct (cf. NCED 181). Note the same spirantization in PC (*ć > s) as in *c (*c > s, see above).

Here Chirikba lists only one PC reflex (*s), without adding any tense variant (*c:). He himself, however, speaks about a possibility of reconstructing _*ćə̄ʒa_ 'marten, weasel' (p. 224 in footnote 4) on the basis of PC _*c:ə̄ʒa,_ Ub. _ćaca_ and Tap. _ʒə̄ʒə̄c._ This is where I reconstruct tense *ć: (see NCED ibid.). Of course, this is just a single example; but the reader should be already used to the uniqueness or rarity of many PWC phonemes. What really matters in a system like this is a combination of distinctive features yielding systematic reflexes.

Although generally the correspondence is OK, the first example given by Chirikba (PC _*sa_ 'yesterday' : Abkh. _*ćə̄_ id.) does actually demonstrate a quite different correlation (PWC *č, see above). Chirikba does not list the Ubykh parallel here, which is quite transparent: Ubykh _ca, wa-cá_ 'tonight'.

7. WC *c^{w} > PC *c^{w}, *c:^{w} : Ub. c^{w} : Abkh. *c^{w}

Just as in the case with *c (see above), Chirikba lumps together two correspondences:

a) WC *c^{w} > PC *s^{w}-, c^{w} : Ub. c^{w} : Abkh. *c^{w}
b) WC *c:^{w} > PC *c:^{w}, Ub. c^{w}, Abkh. *c^{w}.

I can not stop wondering, why Chirikba is ready to accept PWC *ć > PC *s (in _*sa-_ 'yesterday') and PWC *c > PC *s (in _*sa-_ 'get accustomed', see above), but protests against the perfect match:

Ub. _c^{w}a_ 'skin' : PC _*s^{w}a_ id. : Abkh. _*c^{w}a_ id.

Other examples of the same spirantization (ignored by Chirikba) are:

PC _*s^{w}ə̄-ƀ^{w}a-_ 'to hate, envy' : Ub. _c^{w}ə̄-mƀə̄-_ id. : Abkh. _*c^{w}ə̄-mə̄ƀə̄-_ id. PC _*s^{w}a-_ / _*s^{w}ə̄-_ 'drink, suck' : Abkh. _*c^{w}a-_ 'suck' (the Common Abkh. _*s^{w}a-_ 'suck', quoted by Chirikba on p. 259 and reconstructed on the basis of a Bzyp form cited once in Bgazhba 1964, is rather dubious; so is the Shapsygh form _ć^{v}ə̄-λə̄-_ 'suck', apparently recorded by Chirikba from the isolect of his Shapsygh informant in Turkey, and also not attested elsewhere).

As in the case with *ʒ^{w}, these are actually correspondences not for PWC "middle affricates", but for PWC front labialized affricates.

7. PWC *ć̣ > PC *ć̣, *c̣ : Ub. ć̣, c̣, ć : Abkh. *ć̣, *ć

From the abundance of unmotivated "commas" in this row of correspondences it is immediately clear that Chirikba again confuses several rows.

The normal reflexes of *ć̣ are quite symmetrical with reflexes of other palatalized front affricates (or "middle affricates" in Chirikba's terms), i. e. *c̣ in PC (cf. *s/c, *c:, *ʒ / z above), but ć̣ in Ubykh and in Common Abkhaz (cf. Ub. ć, ʒ́, Abkh. *ć, *ʒ́ above). It is easy to observe in most examples adduced by Chirikba (Nos 2, 3, 4, 5, 6, 7, 8, 9, 15; I have some objections against individual aspects of some of those comparisons, but I shall not go into detail right now, because the basic correspondence is correct).

However, Chirikba adds a lot of absolutely heterogeneous examples to the same row of correspondences:

(1) Ub. _ć̣a-_ 'know, learn' : Abkh. _*ć̣a-_ 'to learn' : PC _*ć̣-ʔa-_ 'to know'. Here apparently *ć̣- is preserved in PC because of the cluster *ć̣ʔ(not opposed to *c̣ʔ- in PC), see NCED 262. This is not, therefore, a good case of PWC *ć̣ > PC *ć̣; note also

that when the suffix -ʔa- is absent, PC has a standard *c̣- here (Kab. *c̣ə̄-x^wə̄-* ’be acquainted’).

(13)Ub. *bə̄c̣ə̄-* : Abkh. **bə̄ć̣ə̄-* ’crush’ and (14) Ub. *c̣a-* : Abkh. **ć̣ə̄* : PC **ć̣a-* ’to pass (of time)’ are an absolutely different case, which is easy to see, because Ubykh has here not ć̣, but c̣. This is actually one of the rather numerous examples where Abkhaz has a palatalized ("middle") affricate, corresponding to an Ubykh front affricate, and for PWC *č̣ should be reconstructed here (see above).

(10) Ub. *mə̄ća* : Abkh. **mə̄ćə̄* : PC **ṗc̣ə̄* ’lie’, as well as (16) Ub. *baćə̄* : Abkh. **qa-bə̄ćə̄* : PC **ṗc̣a* ’entire, thick’ actually reflect not *c̣, but *ć. In these cases we have to reconstruct a glottalized *ṗ, resulting in a secondary assimilation in PC (**ṗə̄̃ćV* ’lie’ and **ṗə̄ćV* ’entire, thick’).

(11) Abkh. *jə̄́-c̣a-ǵ(ə̄)* ’more’ is an adverbial formation meaning literally "and under (him, it)", i. e. "additionally" (and is translated into
Russian not as "больше", but as "еще", Germ. "noch"). Anyway, Abkh.
*c̣ can not correspond to Ub. ć (*ća-* ’comparative prefix’) even by Chirikba’s own rules (one more comma should be added: PWC *ć̣ >
... Abkh. *ć̣, *ć, *c̣ !)

(12) Ub. *ć̣a* ’bottom’ : Abkhaz **c̣a* id. Here we see a correlation quite opposite to, e. g., numbers 13 and 14, but still crammed by Chirikba into the same correspondence. The well known Circassian parallel is here ignored by Chirikba - because it does not fit into his scheme entirely. I mean PC **č̣a-* ’below, bottom’ (used as a preverb and exactly matching Abkh. **c̣a-* in the same function).

Here, again, Chirikba is concealing evidence presented by others.
The same correlation between Ub. and PC exists also in:
Ubykh *ṗć̣a-blá* ’dream’ : PC **ṗč̣ə̄-ħaṗa* id.
Ubykh *ṗć̣a-pá* ’leaf’ : PC **ṗč̣á-śa* id.

Abkhaz has peculiar correspondences here: it has **c̣a* ’bottom, below’, but **pə̄xə̄́-ʒə̄* ’dream’, this time -x- corresponding to Ub. ć̣ and PC *č̣. The matter is clarified after adding yet another example:

PC **pč̣á-nṭa* ’sweat’ : Abkhaz **pə̄xə̄-ʒə̄́* id.

It is not difficult to see that we deal here with a rather peculiar correspondence: Ub. ć̣ : PC *č̣ : Abkh. *x before a following affricate, but *c̣ elsewhere (see NCED 182). If we turn to external evidence, we shall see that all of the four examples of this correspondence have very good Eastern Caucasian matches containing exactly one and the same phoneme: not a sibilant, but a glottalized lateral *Ƚ: cf. PEC **HȽŏnŭ* ’bottom’ (see NCED 590-591), **ɦemȽA̐* ’dream’ (NCED 512-513), **Ƚwɨ̆ r̆ʔV* ’leaf’ (see NCED 784-785) and **ɦămȽă* ’sweat’ (NCED 509). The semantic equivalents are absolutely exact, and there is not a single chance of a fortuitous coincidence here.

MOTHER TONGUE

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Both Circassian and Ubykh at present have only one glottalized lateral phoneme, Ƚ; since this phoneme - as well as other modern laterals - is phonetically palatalized, we can safely assume that it was *Ƚ́ in PWC (the palatalization is additionally proved by its Abkhaz reflex, *ŝ). We can equally safely, therefore, reconstruct a non-palatalized *Ƚ in PWC for the row of correspondences PC *c̣̆ : Ub. ć̣ : Abkh. *c̣ / *x (see more below in the section on laterals).

All of this argumentation is completely skipped by Chirikba (who, as we have seen, ignores the PC parallel for 'bottom' and does not even mention the roots for 'dream', 'leaf' and 'sweat' in his book).

8. PWC *c̣^{w} > PC *c̣^{w} : Ub. c̣^{w} : Abkh. *c̣^{w}

This correspondence is correct (see NCED 181), but also with at least one amendment.

Chirikba (pp. 234, 235) compares PC _*c̣^{w}ə̄-_ (in _*c̣^{w}ə̄-c̣a_) 'black' with Abkhaz _*-c̣^{w}a_ (in _*ajə̄k^{w}a-c̣^{w}a_) 'black', which is a traditional comparison, but eliminates Ubykh _ǯa_ 'black' (the comparison with which, he says, is difficult to accept), adding instead Ub. _wə̄-c̣^{w}a_ 'iron' (explained as "black metal"). He forgets to mention, however, that:

a) a very similar correspondence is observed in the common WC word for 'fire' (PC _*mac̣^{w}á_, Ubykh _mə̄ǯá_, Abkh. _*mə̄ca_), although one has to presume a secondary deglottalization in Abkhaz. I would think here of a secondary delabialization (possibly due to the dissimilatory effect of initial *m- in 'fire', with a less clear reason in the Ubykh word for 'black'); since Ubykh has no back labialized affricates, delabialization could result in a shift of the original place of articulation from front to back due to neutralization (*ʒ^{w} could be treated as *ǯ^{w}, delabialised to ǯ). The reason for voicing in Ubykh is not clear.

Despite phonetic difficulties there is little doubt that the words for 'black' and 'fire' (the latter missing from Chirikba's book altogether) should be reconstructed with PWC *c̣^{w}. External cognates have *c̣ in both cases (cf. PEC _*c̣ĂwnV_ 'dark', NCED 352, _*c̣ăjɨ̆_ 'fire', NCED 354-355).

b) Ubykh _wə̄-c̣^{w}a_ 'iron' is, however, quite a different case. The closest parallel for the word (not mentioned by Chirikba, but well known to all Caucasologists) is PC _*ʁ^{w}ə̄č̣ə̄_ 'iron'. Here we have quite a different phonetic correspondence, observed also in: PC _*č̣ə̄_ 'earth' : Ub. _jə̄-c̣^{w}á-_ 'on the earth' (with a secondary deglottalized variant _jə̄-c^{w}á_ id.)

Moreover, _-č̣ə̄_ in PC _*ʁ^{w}ə̄č̣ə̄_ 'iron' is certainly not 'black', but 'blue', cf. PC _*č̣ə̄-x^{w}ə̄_ 'blue' - which leads us to the Abkhaz match _*ja-c̣^{w}a_ 'blue' (having nothing to do with _*ajə̄k^{w}a-c̣^{w}a_ 'black' and its counterparts: PC _*c̣^{w}ə̄-c̣a_ and Ub. _ǯa_). We arrive, therefore, at a new correspondence, not discovered by Chirikba at all:

PC *č̣ : Ub. ç̣^{w} : Abkh. *ç̣^{w}

Just as in the case with PC *č̣ : Ub. ḉ̣ : Abkh. *ç̣/*x (see above), this is a correspondence not fitting into any of the affricate series; fortunately, we have Eastern Caucasian parallels for both words here, and those parallels also have laterals: cf. **lhemƛ̱wɨ̆* 'earth' (NCED 747-748) and **nHǟŁwV* 'blue' (NCED 851-852). The obvious thing to do is to reconstruct here *Ł^{w} for PWC (see more below about laterals).

Back affricates.

1. WC *ǯ > PC *ǯ : Ub. ǯ : Abkh. *ǯ

This row of correspondences can not produce anything but amazement. It is well known that no *ǯ is reconstructable for Proto-Circassian; Kuipers' dictionary does not list a single root with this phoneme. I spent some time trying to identify the source of PC **baǯə* 'to fall', cited by Chirikba as his first example (compared with Abkh. **k^{w}ə-baǯə-* 'to press hard on smth."), but was completely unsuccessful.

In the second (and the last) example (PC **žə-* 'vomit' : Ub. *ǯə-* id.) Chirikba omitted the well known Abkhaz counterpart (Abkh. **ʒ^{w}a-* 'vomit' - which he instead compared with PC **ʒ^{w}a-* 'to dam, fill' and **ħa-(n)ʒ^{w}a* 'haystack' (??), see p. 229).

It is immediately evident that the row PC *ž : Ub. ǯ : Abkh. *ʒ^{w} reflects some labialized PWC phoneme. In NCED 182 I reconstruct here PWC *ǯ^{w} (unfortunately there is a misprint: PAK *ž instead of PAK *ž on p. 182, but the PC form is cited correctly in the body of the dictionary, p. 283, where PWC **ǯ^{w}ə* 'vomit' is compared with PEC **=ǟwčĂ* 'emit, pour, vomit').

This is one of a series of affricate correspondences where Abkhaz has for the most part labialized sibilants (*z^{w}, *ʒ^{w}, *s^{w}; however, *č^{w} > *č, *č̣^{w} > č̣ and *š:^{w} > š), Ubykh has uniformly non-palatalized back affricates or sibilants (č, ǯ, č̣, š, ž), and in Circassian the reflexes are split: we have palatalized back affricates (*č, *č:, *ǯ, *č̣), but non-palatalized fricatives (*š, *š:, *ž). This complicated system is a result of several successive shifts of the features of labialization and palatalization in individual branches, all described in NCED (pp. 185-187) and completely ignored or misinterpreted by Chirikba. For the described set I reconstruct PWC back labialized afffricates and sibilants (*č^{w}, *č:^{w}, *ǯ^{w}, *č̣^{w}, *š^{w}, *ž^{w}) with the following examples:

PC **čədə* 'donkey' : Ub. *čədə* id. : Abkh. **čadá* id. (**č^{w}ədV*)
Ub. *ča-* 'break' : Abkh. **pə-čə-* id. (**č^{w}V-*)
PC **čə-ħa-* 'to roll, wind, wrap' : Ub. *čə-da-* 'twist, spin' : Abkh. **rə-čə* fold, wrap' (**č^{w}ə-*)
PC **čamaǵə* 'sickle' (> Ub. *čamáǵ*) : Abkh. **čəbəgV* id. (**č^{w}VmVgV*)
PC **č:anə* 'sharp' : Ub. *čan-* 'to whet' (**č:^{w}anə*)

PC *p:č:anda-χ^{w}ə̄ 'starling' : Abkh. *z^{w}arə̄d^{w}ə̄-na (*č:^{w}aNə̄d^{w}V)

PC *č̣a 'young, new' : Ub. č̣a 'good' : Abkh. *č̣a 'young, new' (*č̣^{w}a) [Chirikba p. 244, 245 separates the Ubykh form and compares it with Abkh. *čajə̄ 'good' - which is hardly plausible because of lack of glottalization in Abkhaz]

PC *č̣apə̄ 'hemp' : Ub. čə̄ṗ (*č̣^{w}VpV with metathesis of glottalisation in Ub.)

PC *ḳ́ač̣ə̄ 'short' : Abkhaz *ḳ́ač̣ə̄ id. (*ḳ́ač̣^{w}ə)

PC *c̣^{w}a-mə̄č̣ə̄ 'coal' (*'black wood') : Abkh. *mə̄č̣ə̄ 'wood (material)' (*mə̄č̣^{w}ə)

PC *bamə̄č̣a 'tick' : Abkh. *bač̣a id. (Chirikba 245) (*ba(mə̄)č̣^{w}a)

Ub. č̣ə̄- 'press, squeeze' : Abkh. *rə̄-č̣ač̣a- 'squeeze' (*č̣^{w}V-)

PC *žə̄- 'vomit' : Ub. ǯə̄- : Abkh. *ʒ^{w}a- (*ǯ^{w}a-, see above; PC has here a secondary spirantisation, which is why we have *ž, not *ǯ)

PC *maǯá-ga 'dough' : Abkh. *maz^{w}a (*maǯ^{w}a)

PC *šχ^{w}a 'grass; poison' : Ub. šχ^{w}a id. : Abkh. *q^{w}ə̄s^{w}ə̄ 'medicine; poison, powder' (*š^{w}ə̄q:^{w}V, with regular spirantisation in the cluster *šq^{w} > *šχ^{w} in Ubykh and PC; Chirikba 1996 reconstructs Common Abkhaz *q^{w}ə̄š^{w}ə̄, but Marr 1926 cites the Bzyp form as á-χ^{w}ś^{v}, not á-χ^{w}š^{v}, which makes me reconstruct *q^{w}ə̄s^{w}ə)

Ub. šχ^{w}a-bá : Abkh. *s^{w}aqə̄ 'foam' (*s^{w}Vq:^{w}V, with the same spirantisation in Ub. as in the preceding item)

PC *šə̄g^{w}ə̄ 'mountain top' : Ub. šaḳ^{w}a id. (*š^{w}Vḳ^{w}V, with deglottalisation in PC)

PC *naš:a 'melon, cucumber' : Ub. nā̆ša id. : Abkh. *naša id. (*naš:^{w}a)

PC *ma-š:a 'cave' : Ub. ša- 'dig' : Abkh. *ṭə̄-ša 'cave, pit' (*š:^{w}a)

PC *λaš:ə̄ 'strong' : Ub. λaš(ə̄) id. (*λaš:^{w}ə)

PC *ža 'sledge' : Ub. ža 'block, sledge' (*ž^{w}a)

PC *ža- 'wait' : Ub. žə̄- 'endure' (*ž^{w}V; not quite clear is whether Abkh. *la-ǯ́a- 'endure' belongs here or not: secondary delabialisation < *la-ǯ́^{w}a- = *la-ʒ^{w}a-?)

Thus we see that nothing like the simple and beautiful correspondence *ǯ : *ǯ : *ǯ exists in WC; instead, we have discovered a whole series of examples allowing to reconstruct PWC labialized back sibilants. As for PWC *ǯ (as well as *č, *č:, *č̣ etc.), it certainly existed but yielded quite different reflexes (PC *z, Ub. *ʒ, Abkh. *ʒ́, see above).

2. WC *ǯ > PC *ǯ : Ub. ǯ : Abkh. *ǯ

This is the same correspondence (and the same reconstruction) as in NCED 182. It is interesting to note that palatalized back affricates turned out to be much more conservative in WC than their non-palatalized counterparts; note also that back (hushing) affricates are very frequently palatalized in different languages: e. g., all Caucasian languages that possess back affricates but do not possess a distinction in palatalisation, have in fact palatalized back affricates.

Most of the examples produced by Chirikba do indeed demonstrate PWC *ǯ.

There is, however, one root which is quite aberrant (No 2), where Chirikba compares Ad. (not even PC!) *ǯa-* 'hearth, place before the hearth' (in compounds) with Ub. *ǯa* 'back (side)' (cf. also *za(n)-ǯá* 'middle, half'). From Abkhaz Chirikba takes **ǯə̄q̇*^{w}*ə̄* 'hump, humpbacked' - which is rather awkward semantically; to my opinion, a much better match is Abkh. **bə̄ža* 'middle, half'.

Now the problem here is not only with the Abkhaz parallel; it is the PC reconstruction itself that raises doubts. Kabardian has a ź- here (within a perfect match Kab. *źag*^{w} 'hearth' = Ad. *ǯag*^{w}*ə*) instead of an expected **žag*^{w} (< PC *ǯ). This is, in fact, one of the very few Circassian roots where S. Nikolayev has reconstructed PC palatal ("middle") affricates *ć and *ʒ́ (no *ć:^{w} is to be found), in addition to the well established glottalized *ç̣ (usually regarded as *ṣ and being somewhat excessive in the PC system). The common feature of all words with *ć, *ʒ́ is that they develop in Adyghe just as palatalized back affricates (= č, ǯ), but yield middle fricatives in Kabardian (unlike other affricates that yield back fricatives). Cf. Ad. *ǯag*^{w}*ə̄* 'hearth', *ǯa-nə̄q*^{w} 'place before the hearth' : Kab. *źag*^{w} 'hearth'

< PC **ʒ́a*Ad. *pča-də̄ž* 'morning', *pčə̄ħa-śħa* 'evening' : Kab. *pśa-dǯə̄ź* 'morning', *pśəħa-śħa* 'evening' < PC **pća*Despite their rarity, the PC palatal ("middle") affricates reveal quite systematic external parallels: in all cases they correspond to Ubykh palatalized back affricates (which is why Chirikba took this to be a case of PWC *ǯ) and to Abkhaz non-palatalized back affricates or fricatives (see NCED 182). Cf. PC **ʒ́a-* 'hearth, place before the hearth' : Ub. *ǯa* 'back; middle, half' :

Abkh. **bə̄ža* 'middle, half' PC **pća-* 'morning / evening (*dawn)' : Ub. *-ǯ* in *s*^{w}*ə̄-ǯ* 'morning dawn' (*s*^{w}*a- / s*^{w}*ə̄-* 'white, morning dawn'), *z*^{w}*a-psə̄-ǯ* 'evening, twilight' (*z*^{w}*a-psə̄* 'evening') : Abkh. **bə̄čə̄* > **bə̄ǯə̄* (through assimilation) > Tap. *-bə̄ǯ* in *a-la-bə̄ǯ* 'dusk, twilight'

PC **ć̣ə̄-* 'close' : Ub. *č̣a-c*^{w}*a-* id.

PC **ć̣ə̄-* 'do, make' : Abkh. **č̣a- / *č̣ə̄-* id.

This is the series where I have reconstructed PWC palatalized labialized front affricates (**ʒ́*^{w}*a* 'hearth, middle'; **bə̄ć*^{w}*V* 'dawn'; **ć̣*^{w}*V-* 'close'; **ć̣*^{w}*V* 'do, make') for two reasons: a) in Circassian they develop into palatalized ("middle") affricates (this is evidently a shift development: after the original front palatalized affricates had lost palatalization, see above, their labialized correlates lost labialization and filled the free slots); b) in Abkhaz and Ubykh they develop identically with the original palatalized labialized back affricates (*č^{w}, *ǯ^{w}, *č̣^{w}, see below).

3. WC *ǯ^{w} > PC *c: : Ub. ʒ : Abkh. *ǯ^{w}

Chirikba lists only one example here: PC **c:a-ɣa* 'ribs' : Ub. *ʒa-* prev. 'beside' : Abkh. **ǯ*^{w}*a* 'rib, side; prev. beside'. This is basically a correct comparison, although the PC form can not be reconstructed as **c:a-ɣa.* It contains, in fact, a specific rare phoneme that S. Nikolayev has reconstructed as PC *ƛ̣ and which yields ɣ in Adyghe, but ž in Kabardian (cf. Ad. *cāɣa,* Kab. *ʒāža* 'rib'); Kuipers 1975 does not list it at all, considering it to be irregular. Common Abkhaz *ǯ^{w} is in fact, in the same position: it is contained only in one root (listed above), see Chirikba 79. This immediately suggests that the correspondence here is between PC *ƛ̣, Ub. ʒ and Abkh. *ǯ^{w}.

This is, in fact, one of a series of correspondences involving very rare Common Abkhaz phonemes *č^{w}, *č̣^{w} and *ǯ^{w}, notably:

Abkh. *č^{w}ə 'six' : PC *xə, *xa : Ub. *fə* id. Abkh. *č^{w}ə 'lightning, god of thunder and lightning' : Ub. *fa-* preverb meaning 'on the fire, into the fire' [rather mysterious here is PC *šə-bLa 'thunder, lightning, god of thunder and lightning': is *šhere a result of special dissimilative development before the following lateral?]

Abkh. *č̣^{w}a 'thin' : PC *-č̣a in *ṗʔa-č̣a 'thin' : Ub. *ṗc̣a* id. (note here the same front reflex in Ubykh as in the correspondence to Abkh. *ǯ^{w}a; this is why this comparison is both semantically and phonetically preferable to Chirikba's, who compares (p. 248) the Abkh. and PC forms with Ub. *ač̣a* 'wide and flat')

Abkh. *ǯ^{w}a 'side, rib' : PC *-ƛ̣a in *c:a-ƛ̣a 'rib' : Ub. *ʒa-* 'beside'.

Abkhaz has here labialized back affricates; Circassian - either specific velar (or lateral?) reflexes *x, *ƛ̣, or palatalized back *č̣. This suggests that yet another rather enigmatic WC correspondence should be placed within the same series:

Abkh. *ž^{w}əla 'seed; kin, clan' : PC *č:əLá 'seed; village' : Ub. *ǯəǯə* 'seed'.

In the first and fifth of these examples Chirikba (p. 270-271) follows NCED and reconstructs lateral labialized *ƛ^{w} (declaring it, however, "an irregular correspondence") and lateral labialized palatalized *ƛ̣^{w}. It is, however, easy to notice that they are distinguished not so much by palatalization as by tenseness (PC *xə vs. *č:əLá); here Chirikba fails to notice the obvious homogeneity of all the four etymologies.

I reconstruct here PWC palatalised labialised laterals (*ƛ̣^{w}ə 'six', *ƛ̣:^{w}əla 'seed', *Ł́^{w}a 'thin' and *ƛ̣^{w}a 'rib, side').

Here again, in four out of five cases we have reliable EC parallels with lateral affricates, not with back or front sibilants: cf. *ʔränƛ̱E 'six' (NCED 219), *HĭŁ̱īwV 'root, seed' (NCED 571), *=iŁĭlV 'thin' (NCED 639-640) and *ɦireŁwe* 'bone' (NCED 528).

There is, therefore, no *ǯ^{w} in Chirikba's system: what he reconstructs as *ǯ^{w} is *ƛ̣^{w}, and *ǯ^{w} should be reconstructed where he reconstructs *ǯ (see above).

4. PWC *ǯ^{w} > PC *ž : Ub. š^{w} : Abkh. *ʒ́

This phoneme is reconstructed only in one root, the WC word for 'silver' (PC

**t:əžə̄nə̄,* Ub. *daš^{w}anə̄,* Abkh. **raǯə̄nə̄*). This is a rather obscure case (see the discussion in NCED 514), and hardly deserves a special phoneme in PWC - especially since the correspondence does not fit into any of the established systematic rows.

In Chirikba's system palatalized labialized *ǯ^w is especially strange, because the author does not reconstruct *č^w, *č̣^{w} etc. On my reconstruction of palatalized labialized back affricates see below.

5. PWC *č > PC *č, *č: : Ub. č : Abkh. *č

With nine examples this seems at first to be a representative correspondence. Let us, however, look at the examples closer.

Number (2) - the word for "camel" - is certainly a borrowing. Even though the immediate source of PC **maχ(^{w})ča* 'camel' is unknown, the Ubykh and Abkhaz forms are most certainly borrowed from Circassian (this is additionally proved by the irregular correspondence -χ- : -χ- in PC **maχ(^{w})ča* : Abkh. **maχčA*).

Number (7) involves only Ubykh and Abkhaz and fits nicely, as we have seen above, into the row of correspondences for PWC *č^w (see p....). The same is actually true for No (9) - where Chirikba has omitted the PC form **čə̄-ħa-* 'roll, wind, wrap' - which shows that this is a quite different correspondence.

In No 5 Chirikba produces a PC form **ača-* in **ača-x^{w}ə,* **ača-pλə̄* 'sorts of millet', corresponding to Abkh. **ča-ʕ^{w}ə̄-ra* 'harvest; bread'. Kuipers 1975 does not list the word. In Khatanov-Kerasheva 1960 we find the Shaps. form *āša-fə̄* 'sort of millet'; its correspondence to Kab. *āša-x^w* 'mogar (a millet-like plant)' (Kardanov 1957, 21) gives us the PC reconstruction **ača-x^{w}ə̄* with *č, not *č. Therefore, if we have indeed a correspondence between PC **ača* and Abkh. **ča-,* this is one more case of PWC *č^w, see above (with the regular correspondence PC *č : Abkh. č).

No 4: PC **č:ə̄-* (/**č:a-*) 'to run' is compared by Chirikba with Ub. *ča-* 'to fall (of heavy objects)' (?) and with Abkh. **čča-* 'to flow (of water)'. This is all semantically rather dubious. The PC form is best compared with Ub. *ḳa-ča-* 'to walk uncertainly' (see below, including the discussion of the Abkhaz parallel for the root). Abkh. **čča-* 'flow, fall (of water)' can indeed be compared with Ub. *ča-* 'fall (of heavy objects)', but that leaves us again with the reflexes of *č^w.

No 6: PC **ač:a* 'billy-goat' and Abkh. **ča* 'female deer', **ča-c^{w}ə̄* 'male deer'. The Abkhaz word is rather to be compared with Ub. *ž^{w}a* 'deer, female deer'. Semantically the match is certainly better, although phonologically it is also somewhat dubious. We shall see below that Abkh. *č is a regular descendant of PWC *č^w, while Ub. ž^w normally goes back to *ž^w: therefore, this may be a case of old voice variation (**č^{w}a* / **ž^{w}a,* similar to **č^{w}ə̄* / **ǯ^{w}ə̄* in 'brother', but with additional fricativization in Proto-Ubykh). But even if the match Abkh. **ča* : Ub. *ž^{w}a* is incorrect, the Circassian and Abkhaz word still have quite different external parallels (for the former cf. PEC **ʔēǯ́wē* 'goat', see NCED 245, for the latter - PEC **čwĕnʔV* 'game, wild animal', see NCED 350).

No 8 (Ub. *č́aq̇ʷə̄* : Abkh. **čaq̇ʷa* 'mug') is irregular both in Chirikba's and in my system, and can reflect an old variation in labialization / palatalization (Ub. *č́aq̇ʷə̄* presupposes **č́ʷaq̇ʷV,* but Abkh. **čaq̇ʷa* - only **čaq̇ʷV*).

No 1 (PC **čə̄* 'horse' : Ub. *č́ə̄* : Abkh. **čə*) is a well known parallel, but violates the correspondence proposed by Chirikba (he has to say that "the palatal character of č́ in the Ubykh name of horse must be secondary and can be explained by the influence of the Shapsygh form *č́ə̄* 'horse'"which is extremely dubious). This is actually one of a whole series of roots with the correspondence "PC and Abkh. back non-palatalized affricates : Ubykh back palatalized affricates"). Cf.:

PC **čə̄* 'horse' : Ub. *č́ə̄* : Abkh. **čə̄* id. PC **čə̄* 'brother' : Ub. *ǯə̄-λá* id. PC **čə̄-ʡa* / **č̣ə̄-ʡa* 'cold' : Ub. *č́ə̄* id. PC **č:ə̄-* / **č:a-* 'run' : Ub. *-č́a-* in *ḳa-č́a-* 'to walk uncertainly' (in Abkh. one could compare **žžə̄-* 'to overflow', which is, however, somewhat uncertain semantically)
PC **-p:č:a* in **b̌ʷə̄-p:č:a* 'sickle' : Ub. *pč́a-* 'to mow, hew'
PC **č:ə̄rə̄* 'steel' : Abkh. **ǯə̄rə̄* id.
PC **č:ə̄-* 'to cut, cut off' : Abkh. **žə̄-* 'to dig'
PC **ža* 'mouth' (probably with secondary deglottalisation) : Ub. *č̣a* : Abkh. **č̣a*
PC **ʡʷə̄-č̣a-* 'to cut, hew' : Abkh. **č̣a-* 'to hew, chop' [Chirikba 245 tries to add also Ub. *č̣ə̄-* 'to lie (as grass after the rain)' which is extremely dubious for semantic reasons]
PC **č̣Vrə̄ḳʷV* 'turnip' : Ub. *č̣ə̄rḳʷa* 'artichoke' (cf. also Tap. *č̣rə̄ḳʷa* 'turnip', which, however, may be borrowed from Circassian)

Since we have already reconstructed PWC plain back affricates (*č etc.), palatalized back affricates (*č́ etc.), labialized back affricates (*čʷ etc., see above), we have here only one choice: to reconstruct palatalized labialized back affricates (**č́ʷə̄* 'horse', **č́ʷə̄* / **ǯʷə̄* 'brother', **č́ʷə̄* (/**č̣ʷə*) 'cold', **č́:ʷa-* 'run', **pə̄č́:ʷa* 'sickle', **č́:ʷə̄rə̄* 'steel', **č́:ʷə̄-* 'to cut, dig', **č̣ʷa* 'mouth', **č̣ʷa* 'cut, hew', **č̣ʷə̄rə̄ḳʷV* 'turnip'). An additional proof is the variation *čʷ- / *žʷ- in **č́ʷa* 'deer' (see above: the early fricativization here helped to preserve labialization in Ubykh, where the normal reflexes of fricatives *šʷ, *žʷ are šʷ, žʷ with labialisation), as well as the external parallels which in most cases show labialisation (cf. PEC **ɦɨ[n]čwĭ* 'horse', NCED 520-521; **č̣wErHV* 'cold', NCED 393-394, **hilčwE* 'run', NCED 490-491, **č̣ʷVlHV* 'cheek, mouth', see NCED 396).

Note that here, as in the case of most other back and lateral affricates, the PC tense *č: shows a voiced counterpart in Abkhaz (*ǯ or *ž), which separates it from lax *č (= Abkh. *č) and once again proves the phonemic status of tense consonants in PWC.

The whole preceding discussion leaves us with a single example of the correspondence PC *č : Ub. č : Abkh. *č, i. e. No 3: PC **-n-ča* 'privative suffix' : Ub.

-čə̄ id. : Abkh. **čə̄-* 'weak'. Even if the Abkhaz word is related (the analogy between Ub. *psa-q̇a-čə̄* 'weak, feeble' and Abkh. **psə̄-čə̄* 'weak' is rather convincing), in PC we may deal with an irregular development within an auxiliary morpheme: note that Temirgoy has variants *-nča* and *-ǯa,* indicating that *-nča* may be secondarily depalatalized < **-nča,* which would be a good match for Ub. *-čə̄* and Abkh. **čə̄-* (PWC *čʷ).

To sum up: there is not a single good example of the correspondence PC *č, *č: : Ub. č : Abkh. *č , proposed by Chirikba.

6. PWC *č > PC *č, (*č:) : Ub. č : Abkh. *č

The correspondence *č : *č : *č is fine (just as *ǯ : *ǯ : *ǯ, see above); the reconstruction of *č here see in NCED 182. I would also agree with most of the examples (except for Chirikba's removing PC **čxə̄-* 'laugh', obviously equal to Ub. *šʷača-* id., Abkh. **čə̄ča- < *šə̄ča-* id. and replacing it by PC **čə̄ča-* 'to neigh' - which is rather dubious and, anyway, does not have any effect on the reconstruction of *č).

Rather strange, however, is the parenthesized *č: in PC. The author does not list a single example with PC *č: - which is understandable, because there is none. Instead, we have several cases of PC *č: corresponding to Ub. *č and Abkh. *ǯ, cf.:

PC **ɓʷa-p:č:á-* 'dark grey' : Ub. *ɓʷə̄-pča-* 'to get rusty' : Abkh. **ʕʷa-bə̄ǯa* 'dark grey, brown'
PC **p:č:ə̄* 'pike, lance' : Ub. *ča-* 'stake, pole' (in comp.)
PC **p:č:anə̄* 'goat' : Abkh. **ǯə̄ma* id.
PC **p:č:V-* 'plane-tree' : Abkh. **ǯə̄* 'oak-tree'

Chirikba (p. 337-338) tries to explain these cases by secondary voicing *pč- > *bǯ- in Abkhaz, but fails to explain why this voicing is always accompanied by "intensifying" in Circassian. To my view, this is a very normal case of a regular correspondence:

PWC *č: > PC *č: : Ub. č : Abkh. *ǯ

As I noted several times, Abkhaz regularly voices the original tense consonants in the back affricate rows, which is exactly what we observe in this case.

7. PWC *čʷ > PC *š : Ub. ? : Abkh. *čʷ

See above (p...) the arguments in favour of tracing all cases of Abkhaz *čʷ, *č̣ʷ etc. back to original lateral labialized palatalized consonants. The only root given by Chirikba as reflecting *čʷ is Abkh. **čʷə̄* : PC **šə̄bLa* 'lightning, god of lightning', which is one of the cases treated there.

8. PWC *č̣ > PC *č̣, *č̣ : Ub. č̣ : Abkh. *č̣

Here we again observe an unmotivated split PWC *č̣ > PC *č̣, *č̣. It is easy to see that all the cases with PC *č̣ actually conform with the rule of correspondence PC *č̣ : Ub., Abkh. č̣ that I formulated above and that indicates PWC *č̣ʷ. These are examples 6 through 9.

In (1) we have actually a correspondence of Ub. č̣ (*č̣a* 'mouth') to Abkh. *č̣ (**č̣a*) id. which is irregular according to Chirikba himself (he says "the palatalised character of the affricate in Ub. *č̣a* 'mouth' is probably secondary" (?)), but in fact represents a quite regular case of PWC *č̣ʷ, see above.

In (2) (Ub. *č̣a* 'good' : Abkh. *ča-jə̄* id.) the correspondence is again irregular according to Chirikba's own rules (he says: "the affricate in CA **ča-ja* has apparently lost glottalisation"); Ub. *č̣a* should be rather compared with PC **č̣a,* Abkh. **č̣a* 'new, young, fresh' (Chirikba's example No 4) - and this again is a perfectly regular case of PWC *č̣ʷ, see above.

No 3 (PC **ʔʷə̄-č̣a-* : Abkh. **č̣a-* 'hew, chop') is a good case of PWC *č̣ʷ, see above.

Finally, in example No 5 (Ub. *č̣ə̄-* 'press, squeeze' : Abkh. **rə̄-č̣ač̣a* 'squeeze') there is no Circassian reflex, while the Ub. - Abkh. correspondence is again a good case of PWC *č̣ʷ.

In this case, too, it is quite evident that no correspondence like "PC *č̣ : Ub. č̣ : Abkh. *č̣" exists in WC languages.

9. PWC *č̣ > PC *č̣ : Ub. č̣ : Abkh. *č̣

Just as *ǯ:ǯ:*ǯ (PWC *ǯ) and *č:č:*č (PWC *č) this is a well established correspondence and reconstruction (see NCED 182), and I do not have any objections except some minor etymological points (e. g., Ub. *čə̄* 'cold' belongs rather to PC **čə̄-ʔa / *č̣ə̄-ʔa* id., see above, than to PC **č̣ə̄* 'winter' - these roots in PC should be strictly kept apart).

10. PWC *č̣ʷ > PC *č̣ : Ub. č̣ : Abkh. *č̣ʷ

This correspondence (based on one word: "thin") was dealt with above (p. ...), where I tried to show that it in fact represents PWC lateral *Ł́ʷ; see also above for the arguments that in this row of correspondences Ub. has not č̣ (*ač̣a* 'wide and flat'), but c̣ (*ṗc̣a* 'thin'), which Chirikba for some reason "finds less plausible".

I hope to have shown above that the system of affricates reconstructed by Chirikba does not account for a huge amount of cases and should be in fact rewritten completely. We find abundant evidence in Western Caucasian languages for reconstructing two rows of affricates with complete distribution of the features of palatalisation and labialisation, namely:

*c	*c:	*ʒ	*c̣	*č	*č:	*ǯ	*č̣
*ć	*ć:	*ʒ́	*ć̣	*č	*č:	*ǯ	*č̣

*c^{w}	*c:^{w}	*ʒ^{w}	*c̣^{w}	*č^{w}	*č:^{w}	*ǯ^{w}	*č̣^{w}
*ć^{w}	[*ć:^{w}]	*ʒ́^{w}	*ć̣^{w}	*č́^{w}	*č́:^{w}	[*ǯ́^{w}]	*č̣́^{w}

Fricatives

1. Labial fricatives : *f

Rather surprising is Chirikba's reconstruction of PC *f (p. 116), based on a single Adyghe word *č̣a-fafə̄-č̣ə̄-* 'to flutter', without a Kabardian correspondence. It is well known that the only source of Ad. f is PC *x^{w}, therefore formulating the correspondence as PWC *f > PC *f : Ub. f : Abkh. *f seems rather strange (in NCED I reconstruct: PWC *f > PC *x(^{w}) : Ub. f : Abkh. *f). Other objections: in the root for 'smell' I prefer to reconstruct *b^{w} (see above), comparing not the isolated Shapsygh *fa-ma,* but the Common Circassian **ba-ma* 'smell'; for PWC **fV* 'eat' cf. also PC **-xa-* in **č-xa-* 'eat'.

2. Sibilant fricatives.

In the system of fricatives Chirikba reconstructs more distinctions than in the system of affricates. Thus, he reconstructs an opposition of labialized front fricatives (*s^{w}, *z^{w}) opposed to labialized middle fricatives (*ś^{w}, *ź^{w}), as well as to labialized back fricatives (*š^{w}, *ž^{w}) and labialized palatalized back fricatives (*š́^{w}, *ž́^{w}). Unlike the system of affricates, this seems closer to my reconstruction presented in NCED; a closer inspection, however, reveals a lot of differences that are to be discussed.

PWC *z > PC *z, Ub. z, Abkh. *z

This is the same correspondence as in NCED 181 (with a minor difference: I think that in Ubykh we have to acknowledge a variation of z and ʒ; for examples see above, p...).

PWC *z^{w} > PC *ž́ : Ub. ź : Abkh. *ž^{w}

This is a completely new correspondence and reconstruction (Chirikba's labialized front consonants do not correspond to mine) and it is based on two examples. One of them is PC **ž́V-* : Ub. *aź-q̇a* 'right (side)'; Chirikba omits the Abkhaz counterpart - which is, as a matter of fact, not **ž^{w}a,* but **aɣ́a* 'right'.

With PC **ž́ə̄,* Ub. *źə̄* 'to comb' Chirikba compares Abkh. **ž^{w}a* 'to scutch, swingle, tear in pieces'. However, here we have a perfect match (just as in the case with 'right') with Abkh. *ɣ́ : Abkh. **ɣ́a-* 'to scrape' (see the discussion in NCED 495).

Thus in both cases we have a quite different correspondence: PC *ž́ : Ub. ź : Abkh. *ɣ́, where *z^{w} is rather hard to reconstruct. Other examples: PC **bža* 'bee' : Ub. *bźā-mla* 'hornet, big wasp' (Chirikba p. 255 compares the Ub. word with Abkh. **bə̄źə̄-*

_bə̄źə̄_ 'to say lies' (???))
PC _*žə̄_ 'wind, air' : Abkh. _*ɣ́ə̄_ 'swift'.
PC _*bžə̄_ 'stateliness' (in _*bžə̄-s^{w}a_, _*bžə̄-ç^{w}a_ 'stately', _*bžə̄-k:^{w}a_ 'unstately') : Ub. _źə̄-_ 'to put on flesh', _a-ź-q̇a_ 'fat, corpulent' : Abkh. _*ɣaɣa-_ (probably assimilated < _*ɣ́a-ɣa-_) 'branchy, spacious; fat, plump (of man)' (Chirikba p. 255 compares Ub. _źə̄-_ - without mentioning the PC form - with Abkh. _*-źə̄_ suffix meaning 'awkwardly excessive' - which is rather dubious).

For this correspondence I have reconstructed PWC *ɣ́ (for a discussion of Chirikba's *ɣ́ see below).

PWC *s > PC *s : Ub. s : Abkh. *s

No objections in this case (the correspondence is identical to the one in NCED p. 181).

PWC *s^{w} > PC *s : Ub. š^{w} : Abkh. *ś

Note a complete difference of reflexes of *z^{w} and *s^{w} in Chirikba's system (*ž : ź : *ž^{w} vs. *s : š^{w} : ś), which suggests that the rows are actually heterogeneous.

In fact, here (as in some other cases) Chirikba confuses three different sets of correspondences: a) PC *s : Ub. ś : Abkh. *ś (where I reconstruct *ś); b) PC *s : Ub. s : Abkh. *ś (where I reconstruct a back fricative *š, see above on the reconstruction of *č, *ǯ, *ç̌); c) PC *ś : Ub. š^{w} : Abkh. *ś (where I reconstruct a front palatalized labialized fricative *ś^{w} - palatalisation being preserved in PC and Abkh. and labialization being preserved in Ubykh).

Let us look at the examples:

Numbers 1, 2, 3, 6, 7, 8 involve only comparisons of Circassian and Abkhaz (PC *s : Abkh. *ś), without any Ubykh parallels - where, therefore, one can reconstruct *ś or *š. In fact, in No 6 (PC _*sə̄_ 'swim' : Abkh. _*ʒə̄-śa_ id.) there is an Ubykh parallel, omitted by Chirikba, and it is Ub. _ʒ́a-śa_ 'swim', with nothing like -š^{w}- (the root clearly should be reconstructed as _*śa_).

Numbers 9, 10, 11 involve only comparisons of Ubykh and Abkhaz (Ub. š^{w} : Abkh. *ś), without Circassian parallels, where, therefore, one can safely reconstruct *ś^{w}.

This leaves us with just two rather dubious cases:

(4) PC _*sa_ 'sabre, knife' : Ub. _aš^{w}a_ 'sickle' : Abkh. _*aśa_ 'sword'. Here Chirikba himself proposes an alternative etymology: Ub. _aš^{w}a_ 'sickle' : Abkh. _*aš^{w}a_ 'to weed' (PWC _*aš^{w}a_). We are left with PC _*sa_ : Abkh. _*aśa_ that again points either to *ś or to *š.

(5) PC _*cə̄_ 'hair, wool' : Ub. _daš^{w}ə̄_ : Abkh. _*laśa_ 'wool'. The Circassian parallel here (suggested by Abdokov: _*cə̄_ < _*Tsə_) is very dubious. The Ubykh and

Abkhaz form as such point again to PWC *ś^{w}; even if the Circassian form belongs here, one has to suspect a secondary deformation of the fricative within the newly formed cluster *Ts(́). [Note that Chirikba's discussion of the initial consonant here is completely strange: no development *r- > *l- had ever taken place in ProtoAbkhaz; related East Caucasian forms do not show any "fluctuation" in in the anlaut consonant, but correspond perfectly well to each other, going back to *lāswE, see NCED 743; Darg. dus and Tab. dis 'wool, fleece' do not exist, and the Lezg. form is not jus, but jis; Tsakh. jis means not 'goat skin', but 'goat wool'; Av. ras, Godoberi ras do not correspond to Lezg. jis and Tsakh. jis that go back to a different root, PEC *ħāls̱V, see NCED 500.]

We can see that neither *z^{w} nor *s^{w} that Chirikba reconstructs are valid
PWC phonemes. In fact, one can safely reconstruct *z^{w} and *s^{w} where
Chirikba reconstructs *ź^{w} and *ś^{w} (see below). PWC lacked a distinction between "front" and "middle" sibilants, that Chirikba is willing to project into the Common WC stage: this is an excessive feature, and the whole PWC system is sufficiently well explained by postulating the opposition of front and back affricates / fricatives, as well as the features of labialization and palatalization.

PWC *ź > PC *ź : Ub. ź : Abkh. *ź

One more example of a beautiful, but non-existant correspondence.

Chirikba's evidence is:

PC **źaźa* 'slow, lingering' : Abkh. **źaźa* 'dullish, slow'. Since there is no Ubykh parallel, it may well be a case of the correspondence "PC *ź : Ub. ž^{w} : Abkh. *ź", for which (see NCED 182) I reconstruct *ź^{w}. Chirikba is quite right in saying that "Ashkh. *ź^{v}aź^{v}a* is a borrowing, as Kab. ź in loans is usually rendered by Ashkh. and Tap. ź^{v}" (p. 255). But this very fact is quite eloquent: in fact, the phoneme ź in PC always goes back to labialised *ź^{w} or *ž́^{w}, and the fact that relatively recent Abaza loans from Circassian preserve the labialization show that it had disappeared very late. The Ashkh. borrowing *ź^{v}aź^{v}a* is, therefore, a good proof for reconstructing *ź^{w} for this root.

PC **źa-* 'tuberculosis, consumption' : Ub. *źa-ɓ^{w}a* 'illness' : Abkh. **źə̄-ʕ^{w}a* 'plague, sickness'. See above (p...) about the root **źV* 'ill' (> PC **wə̄-zə̄,* Ub. *źa-ɓ^{w}a,* Abkh. **č̌ə̄-ma-za-*). Abkhaz **źə̄-ʕ^{w}a* may, in fact, correspond to PC **źa-,* but I think that Kuipers 1975, 28 is right in explaining the root here as 'roast, be scorched' (PC **źə̄- / *źa-*). The latter normally corresponds to Abkh. **ʒ́ə̄-* 'roast, bake' (and **źə̄-ɓ^{w}a* may well be a result of the rather frequent variation in fricativity) and Ub. *ž^{w}a-* id. < PWC **ź^{w}a* 'roast, bake, scorch'. There may have been some secondary interaction between the two roots (**źV* and **ź^{w}a*), but in general they are quite distinct.

PC **źaźə̄ja* : Ub. *źāźa* 'kidney'. Both Ub. *źāźa* and *źaźə̄ja* are most certainly borrowed from Circassian: the genuine Ubykh word for 'kidney' is *č̣ā̆č̣a,* exactly corresponding to Abkh. **č̣ač̣a* id. Since *-źə̄ja* in PC is a normal diminutive suffix, we may think of an irregular development in PC **č̣ač̣a-źə̄ja* > **č̣a-źə̄ja* > **źaźə̄ja* through assimilation.

PC **-źƀə̄* 'husk' : Ub. *źƀə̄źƀə̄źƀa* 'the sound of turning of the mill-wheel'. An obviously onomatopoeic and even so very dubious semantically comparison.

Ub. *bźā̆-mIa* 'hornet, big wasp' : Abkh. **bə̄źə̄-bə̄źə̄* 'to say lies'. Very dubious semantically; cf. also a far better match for the Ubykh form in PC **bža* 'bee' (< PWC **bə̄ɣ́a*).

Ub. *źə̄-* 'to become fat, put on flesh', *a-ź-q̇a* 'fat, corpulent' : Abkh. **-źə̄* suffix with the meaning 'awkwardly excessive'. A different etymology of the Ubykh form (presupposing PWC *ɣ́) see above, p...

In my system PWC *ź is reconstructed for the correspondence "PC *z :
Ub. ʒ́ / ź : Abkh. *z", which is refuted by Chirikba. Examples (PWC **mə̄źə̄* 'prickle, pine'; PWC **źV* 'ill'; PWC **ṗə̄źV* 'clean') see above, p.... In PC and Ubykh the development of *ź is quite consistent with the rest of palatalized front affricates and fricatives (which are preserved as "middle" sibilants in Ubykh, but yield plain front sibilants in PC); in Abkhaz the development is exceptional (other palatalized sibilants usually yield "middle" sibilants, just like in Ubykh), but the row of correspondences still fits rather nicely into the general pattern of development of the PWC palatalized sibilants.

PWC *ź^{w} [*z^{w}] > PC *z^{w} : Ub. z^{w} : Abkh. *z^{w}

This row of correspondences is quite correct - except for its interpretation. Chirikba reconstructs here "middle" labialized *ź^{w}, while I treat it as reflecting just plain front labialized *z^{w}. In my system the palatalized labialized *ź^{w} is reconstructed for a quite different set (PC *ź : Ub. ž^{w} : Abkh. *ʒ́ / *ź, cf. PWC **ź^{w}V* 'to roast, bake', PWC **ź^{w}ə̄* 'kid', PWC **bə̄ź^{w}a* 'horn, hoof', PWC **ź^{w}aź^{w}a* 'slow' - see above, pp....).

PWC *ś > PC *ś : Ub. ś : Abkh. *ś

This correspondence deserves the same analysis as *ź : ź : *ź. Here we have, in fact, a confusion of several heterogeneous correspondences ("combed" by Chirikba and made to look as an elegant match of identical phonemes).

No 5 (PC **k:^{w}ə̄śa* 'cradle' : Abkh. **k^{w}aśə̄-k^{w}aśə̄* 'go at a jog-trot') is a very dubious parallel and was refuted above (p....) because of the irregular correspondence *k:^{w} : k^{w}.

In numbers 4 (PC **śa* : Abkh. **śə̄* 'weave'), 5 (PC **śə̄-na* : Abkh. **śə̄*'lamb'), 7 (PC **-śa* : Abkh. **-śa-* in body parts), 12 (PC **śa-śa* 'crumble' : Abkh. **śa-śa* 'small, mince') we have no Ubykh parallels: the correspondences, therefore, fit well into the row "PC *ś : Ub. šʷ : Abkh. *ś" where I reconstruct PWC *śʷ (see NCED 182).

No 8 (Ub. **pśa-* 'swell' : Abkh. **pśə̄-la* 'fat') is, indeed, a case of PWC *ś - however Chirikba here (as often) omits the Circassian parallel which is PC **Psə̄-ta-* 'to swell' (cf. also **Psa-sʷə̄* 'pregnant (of animal)') and has not *ś, but *s (thus conforming to my correspondence in NCED 181).

The same is true for No 10 (Ub. *məśa* 'call, read' : Abkh. **mə̄śə̄* 'to swear'), although here the comparison itself may be wrong. The Ub. form should rather be compared with Abkh. **pə̄x́a-* 'call, read' < PWC **pə̄x́a* with a quite different phoneme. [The probable PC match here is rather complicated: one would expect a *š < *x́, but in fact we have a variation **pšə̄- / *psə̄-* in Ad. *pšə̄-sa,* Kab. *psə̄-s* 'tale'; this is probably due to dissimilations with the second part of the word].

We can also safely reconstruct *ś for No 11 (Ub. *-śa* gerundial suffix : Abkh. **-śa* adverbial suffix) - but there is no PC parallel with a *ś here, as well.

No 2 is a rather complicated case. The standard form of the PC interrogative pronoun 'what' is **sə̄-* (Ad. *sə̄-da,* Kab. *sə̄-t*), which corresponds nicely to the standard Ubykh *sa* 'what' (and to PEC **s̱āj* 'what', see NCED 958) and allows to reconstruct PWC **sV.* Dialectal forms in Adyghe (Bzhed., Shaps. *śə̄-də*) are still unexplained (the correspondence within Circassian is absolutely irregular) and may reflect either an irregular development within an auxiliary morpheme, or a quite different original root, contaminating with **sV.* Anyway, it is very difficult to base a reconstruction on evidently irregular and exceptional cases.

Instead of 12 examples we thus have only 3 demonstrating Chirikba's rule (*ś : ś : *ś):

(1) PC **wə̄-śa* 'to stuff, fill, beat, crush' : Ub. *śaśə̄-* 'to beat, pound, crash' : Abkh. **śə̄-* 'to hit, beat'. The attribution of the PC form here is not quite clear: it may rather belong together with Abkh. **šʷa* 'destroy, dismantle' (although **sʷa* also can be reconstructed, the Bzyp form being unknown). The actual relic of the root corresponding to Abkh. **śə̄-* is rather found within the exact match of Abkh. **gʷə̄ śə̄*'to take offence' (lit. 'hit the heart') : PC **gʷə̄-sa-* id. Thus here we have again the correspondence "*s : ś : ś".

(9) PC **pśaśa* 'girl' : Ub. *śasa* 'bride'. This is originally a root with two different fricatives, subject to various assimilations: cf. in Ubykh also the variant *sasá,* in Ad. - *sās* (see the discussion in NCED 969).

(3) PC **pśə̄* 'get tired' : Ub. *pśa-xʷə̄-* 'breathe' : Abkh. **pə̄śə̄-* 'die'. This is the only unobjectionable example of the correspondence *ś : ś : *ś". Ubykh should regularly have šʷ (< *śʷ) here, and the irregular reflex must be explained by an early dissimilative delabialisation (**pə̄śʷa-xʷə̄- > *pə̄śa-xʷə̄ > pśa-xʷə̄-*), see NCED 961.

To sum up: instead of Chirikba's "ś : ś : ś" we in fact have two different sets of correspondences:

a) PWC *ś > PC *s, Ub. ś, Abkh. *ś (cf. **pə̄śV* 'swell', **śa* 'gerundial suffix', **śV* 'beat, hit' above, as well as **śa* 'swim', see p....); cf. also:
PC **-sa* 'think' (in **Psə̄-sa-*) : Ub. *śa-* id.
Abkh. **ç̣ə̄śə̄* 'young, child' : Ub. *ç̣ə̄śə̄* id.

b) PWC *ś^{w} > PC *ś, Ub. š^{w}, Abkh. *ś (cf. **ś^{w}V* 'weave', **ś^{w}ə̄* 'lamb', **ś^{w}aś^{w}a* 'crumble, mince' above, as well as **laś^{w}V* 'wool', see p...); cf. also:
Ub. *š^{w}a-* 'shear, shave' : Abkh. **śa-* id.
Ub. *pχ́aš^{w}ə̄* 'woman' : Abkh. **pə̄ħ^{w}ə̄śə̄* id.

PWC *ś^{w} [*s^{w}] > PC *s^{w} : Ub. s^{w} : Abkh. s^{w}

Just as in the case with its voiced counterpart (see above, p....) this correspondence is quite correct. I reconstruct here PWC *s^{w}; for the reconstructed *ś^{w} in my system see the argumentation above.

PWC *ž > PC *ž : Ub. ž : Abkh. *ž

This is one more fictitious correspondence.

From the etymology No 1 (PC **ža* 'sleigh' : Ub. *ža* 'block, log' one should remove Abkh. **(a)ža* 'ash-tree' - which corresponds quite nicely to Ub. *ž^{w}a* 'willow, osier' (< PWC **Ľ^{w}a,* see below). We are left with two examples of PC *ž : Ub. ž and with two examples of PC *ž : Abkh. *ž, without a single case of *ž : ž : *ž. Chirikba also adds (under the heading "irregular correspondence") two cases of PC *ž corresponding to Ubykh ž.

In effect, there are two different sets of correspondences:

a) PWC *ž́ > PC, Abkh. *ž : Ub. ž́. Cf.:

PC **bžə̄-ʒa* 'flea' : Abkh. **bə̄žə̄* 'beetle' (example No 3 in Chirikba's *ž);

PC **žaq^{w}ə̄* 'belch' : Abkh. **žaħ^{w}a* 'chew the cud, ruminate' (example No 4 in Chirikba's *ž);

PC **g^{w}ə̄-bžə̄-* 'become angry' : Ub. *ǵə̄-bžə̄-* id., *c^{w}a-bž* 'mistake, sin' : Tapanta *g^{w}-bžá-ra* 'distress, anxiety', *bžá-ra* 'defect, fault' (Chirikba lists the PC-Ub. match in his "irregular correspondence", but transfers Ub. *c^{w}a-bž* into a different etymology, see below, and omits the Tapanta parallel);

PC **bžə̄-ħa* 'autumn' : Ub. *bža* 'winter' ("irregular correspondence", according to Chirikba);

Below I intend to show that no correspondence like "*ž : ž : *ž" exists in Western Caucasian languages (again despite Chirikba's fictitious correspondences). Original palatalized back fricatives were preserved in Ubykh, but (unlike affricates) underwent depalatalization in PC and Abkhaz. The listed examples, therefore, perfectly regularly reflect PWC *ž.

b) PWC *ž^{w} > PC, Ub. ž, Abkh. (?) *ʒ́
Cf. _*ž^{w}a_ 'log' and _*ž^{w}a-_ 'to wait, endure' on p....
The Abkhaz reflex *ʒ́ is somewhat dubious here. On analogy with *š^{w} > *š something like *ž would be expected, but the only example is Abkh. _*ʒ́a_'to endure', and I have to list the reflex *ʒ́ (with a question mark) in NCED 182.

As for the original PWC *ž, this phoneme (like all other plain back sibilants) changed to a front *z in Ubykh and Circassian, but to a "middle" *ź in Abkhaz (cf. _*žə̄_ 'gall', _*žə̄-_ 'reciprocity prefix', _*žV-_ 'gather' on p...).

WC *ž > PC *ž : Ub. ž : Abkh. *ž

Let us look at Chirikba's data here:

(1) PC _*bžə̄-ɓa_ 'grease-stain' : Ub. _c^{w}a-bž_ 'mistake, sin' : Abkh. _*bžə̄-xə̄_'corrupt, spoil'. This is a complete misunderstanding: PC _*bžə̄-ɓa_ is a regular derivate from _*bžə̄-_ 'to roast (on oil, grease)' (see Shagirov 1, 90); the Ubykh form belongs together with Ub. _ǵə̄-bžə̄_'become angry' to PWC _*bə̄žə̄_ (> Abkh. _*bža-,_ see above). Finally, Abkh. _*bə̄žə̄-_ 'spoil(ed), corrupt' - since Abkh. *ž can only go back to PWC *L̓ - is best compared with PC _*bLa_ in _*ɓa-bLá_ 'hunger; crop failure' (lit. 'year failure').
(2) The second component in PC _*čadə̄-bžə̄_ 'bellows' is unclear, and its comparison with Ub. _bžə̄-_ 'to melt, fuse' is absolutely uncertain. In NCED 627 Ub. _bžə̄-_ is compared with PC _*z^{w}ə̄-_ 'melt, thaw', with a supposition of secondary delabialization in Ubykh, which is also not very secure.
(3) PC _*jaža-_ 'to go, leave' : Abkh. _*ǵažə̄-_ 'to turn round' - evidently a semantically very weak match.
(4) PC _*žə̄_ 'early' : Abkh. _*ša-žə̄_ 'early morning'. The match looks superficially fine: however, in Abkhaz _*ša-žə̄_ obviously goes back to _*ša-žə̄_ as a result of assimilation (just like _*ša-bə̄žə̄_ 'midday' goes back to the same _*ša-_ 'day' + _*bə̄žA_ 'middle'); _*žə̄_ 'quick, young' can be found in Abkh. _a-žə̄-ć^{V}_'young boy', _a-žə̄ʕ^{w}-ħ^{w}a_ 'quickly'. The correspondence is, therefore, irregular both in my system and in Chirikba's; one wonders if the PC form is not etymologically identical to PC _*žə̄_ 'wind, air' (as "quick movement", cf. the Abkhaz parallel _*ýə̄_ 'swift', see above, p....).
(5) (6) The matches between PC _*jaža_ 'ashes' and Abkh. _*žə̄-jə̄_ 'smith', as well as between PC _*bžə̄_ 'yoke' and Abkh. _*bža-_ 'to tame' are weak semantically.

It is not surprising that none of the 6 examples presented can be met in any of the etymological literature on WC languages: the matches are all rather weak semantically and were evidently invented just to demonstrate the non-existing correspondence "ž : ž : ž".

On my reconstruction of PWC *ž see above.

PWC *ž^{w} > PC *ž : Ub. ž^{w} : Abkh. *ž^{w}

This is a surprising correspondence. No PC forms at all are given (so where from does Chirikba get the PC *ž ?). There are only two examples:

a) Ub. _ž^{w}a_ 'willow' : Abkh. _*ž^{w}a-_ 'to tear' (even Chirikba places a question mark here). The Ubykh form, as I have written above (p...) is a good match for Abkh. _*ža_ 'ash-tree' (going back to _*L̕^{w}a,_ see below);

b) Ub. _ž^{w}ə̄_ : Abkh. _*ž^{w}a_ 'ten'. This correspondence is quite exceptional, and the Circassian match is PC _*ṗć̣ə̄_ (not _*ṗc̣a,_ as cited in Chirikba 263). The only explanation I can think of (see in NCED 246) is a reconstruction of PWC _*b-ć̣^{w}ə̄_ 'ten', with a regular development into PC _*ṗć̣ə̄,_ but with secondary voicing in Abkh. and Ub. Ubykh _ž^{w}ə̄_ normally reflects _*ź^{w}ə̄,_ but the Abkhaz reflex must have been _*ža,_ changed to _*ž^{w}a_ under the influence of the neighbour numeral _*z^{w}ə̄_ 'nine' (the interaction between 'nine' and 'ten' is not an unfrequent phenomenon, cf. Slavic _*devẽtь_ 'nine' instead of _*nevẽtь_ under the influence of _*desẽtь_ 'ten' etc.).

Independently of the way we treat the PWC root for 'ten', it is clear that nothing like the correspondence proposed by Chirikba exists. For my reconstruction of *ž^{w} see above, p....

PWC *ž^{w} > PC *ź : Ub. ž^{w} : Abkh. *ž^{w}

This correspondence, based just on one example (PC _*źə̄_ : Ub. _ž^{w}ə̄_ : Abkh. _*ž^{w}ə̄_ 'old') is, surprisingly enough, quite correct and just the same as in NCED 182. I can add another example of the same correlation: PC _*g^{w}ə̄-źə̄-_ 'old hidden anger' : Ub. _ǵə̄-ž^{w}_ 'revenge' : Abkh. _*g^{w}ə̄-(pə̄)-ž^{w}ə̄_'anger, hidden anger' (a compound with *ǵ^{w}ə̄- 'heart' in all three subgroups).

PWC *š > PC *š, *š: : Ub. š : Abkh. *š

This is an interesting case, with most examples superficially looking very convincing. One can quickly note, however, that the vast majority of Circassian parallels here has *š:, not *š, cf. _*ma-š:a_ 'hole', _*(b)b́^{w}ə̄-(p)š:ə̄_ 'millet-straw', _*š:a_ 'to ripen', _*š:a_ 'raw internal fat'. From these, moreover, some reveal different patterns of correspondences in Ubykh (for _*š:a_ 'fat', Abkh. _*š(ə̄)ša_ cf. Ub. _s^{w}ə̄-q̇a_ 'butter, fat'; for _*(p)š:ə̄_ 'millet-straw', Abkh. _*pšə̄_ 'millet' cf. Ub. _pšə̄_ 'millet').

The only two examples with PC *š are:

(5) PC **ša-ḳ́ə̄* 'cloth, textile' : Abkh. **šə̄* 'white linen'. The Abkhaz word means actually 'white' (as a noun: 'white linen', 'wall-eye') and corresponds to PC **xʷə̄* 'white' (< PWC **λʷə̄*). A much better match for the PC root is Abkh. **sʷə̄-* 'to put on clothes'.

(6) PC **šχʷa* 'grey, blue; grass' : Ub. *šχʷa* 'grass'. Both the PC and Ub. forms mean also 'poison' and correspond to Abkh. **qʷə̄sʷə̄* 'medicine, poison' (see above, p...).

We have, therefore, two clearly different (though similar) correspondences: a) PC *š : Ub. š : Abkh. *sʷ (where I reconstruct *šʷ) b) PC *š: : Ub. š : Abkh. *š (where I reconstruct the tense correlate *š:ʷ).

Once more we see that Chirikba's ignoring the tense/lax opposition in PC leads him to quite wrong results.

As for *š proper, in my system it is reconstructed for "PC, Ub. s : Abkh. ś" (see above, p...).

PWC *š́ > PC *š́, *š́: : Ub. š́ : Abkh. *š́

Just as in the case with *ž́, this correspondence is completely misleading. Let us look at the examples:

(1) PC **š́ə̄-* locative prefix: Ub. *š́a-* in *š́a-χ́a* 'until'. A much better match is Ub. *śa-* locative prefix and root ('above'; 'inside').

(2) PC **š́:ə̄š́:ə̄-* 'neigh' : Ub. *š́ə̄š́ə̄-* id. An onomatopoeic root, most probably borrowed in Ubykh from Circassian.

(3) The comparison of PC **š́ə̄š́ə̄-* 'belong to, be a part of' (uncertain, if *š́ or *š́: is to be reconstructed) with Ub. *š́ə̄-* 'be, become' belongs to Dumézil 1975 and is rather doubted by Shagirov (2, 108). In NCED 663 I have proposed to compare Ub. *š́ə̄-* with Abkh. **xa-* 'to become', thus reconstructing PWC **xV-* (cf. further PEC **=ī xA* 'be, become').

(4,5,9) These are parallels between grammatical suffixes that often violate regular correspondences. All of them involve only parallels between Ubykh and Abkhaz (only in No 9 the Adyghe ergative suffix *-š́* is for some reason compared with Abkh. **š́a-wə̄-ḳə̄* 'somebody' and Ub. *ma-š́a* 'everybody' - an altogether very dubious example). Moreover, in No (5) (Abkh. **-jə̄-š́ə̄* 'suffix of imperative and request') one may suppose a secondary assimilation in palatalisation to the preceding -j- (**-jə̄-š́ə̄* < **-jə̄šə̄* which normally would be expected).

(6) Ub. *š́ə̄š́ə̄-* : Abkh. **š́ə̄-* (**š́ə̄š́ə̄-*) 'caress, stroke'. The Abkhaz word corresponds well to PC **λa-* 'to sharpen, whet; to stroke' and thus goes back to PWC **ƛV-;* the Ubykh form can be an Abkhaz loanword.

(7) PC **pš́:ə̄-* 'to measure (dry substances)' : Ub. *pš́ə̄-* 'to measure'. Very probably a Circassian loanword in Ubykh.

(8) PC _*pš̌:ə̄_ 'feudal prince; father-in-law' : Abkh. _*apš̌ə̄_ 'big, strong, great'. The Abkhaz word is only found as a second part of compounds and usually treated as _pš̌ə̄-_ 'red' (cf. _a-zaarapš̌_ 'terrible anger' = 'red anger', _á-matapš̌_ 'a very venomous snake' etc.). In Tapanta it is only attested in _q̇abard-apš̌_ 'the Great Kabarda'. It is thus rather probable that in some cases it is actually = Abkh. _*pš̌ə̄_ 'red', in others can represent a later loan from Circ. _pš̌ə̄_ (cf. especially the case with _q̇abardapš̌_).

Original *š̌, *š̌:, just like *ž̌, have preserved their palatal nature in Ubykh, but were depalatalized in Abkhaz and Circassian. Cf.:

PC _*šə̄-(pa)-_ 'to collect, pick up one by one', _*ša-pχa_ 'measure, size, example', _*ša-ta-_ 'to try out, check' : Abkh. _*ša-śə̄_ 'deposit, pledge; hostage', _*ša-ta-_ 'to spare (e.g. smb.'s life)';
PC _*š:ə̄-_ 'to measure' : Ub. _š̌ə̄-_ 'to divide', _š̌ə̄_ 'part' : Abkh. _*ša-_ 'to divide; to measure'
PC _*(b)b́^{w}ə̄-(p)š:ə̄_ 'millet straw' : Ub. _pš̌ə̄_ 'millet' : Abkh. _*pə̄šə̄_ id.

There is only one example of *š̌ (which is not surprising, because in PC *š is an extremely rare phoneme), and two reliable cases of *š̌:; however, the correspondences fit well into the overall scheme of development, and the reconstruction of *š̌ and *š̌: in these cases seems reliable.

PWC *š^{w} > PC *š, *š: : Ub. š^{w} : Abkh. *š^{w}

Just as the postulated by Chirikba voiced counterpart *ž^{w} (see above), this correspondence simply does not exist. Let us look at the examples:
(1) PC _*š:a-_ 'hunt', _*š:a-ḳ^{w}a_ 'hunter' : Ub. _š^{w}a-ḳ́a_ 'hunter' : Abkh. _*š^{w}a-ra-_ 'hunt; wild animal'. Let me note at once that Ub. _š^{w}a-ḳ́a_ is a mistake: Ubykh has, indeed, _-š^{w}a_ in _λa-š^{w}á_ 'hunting (adv.)' (where _λa-_ is 'deer'), but the word for 'hunter' is _s^{w}ā-ḳ́á_. Chirikba here confuses two different roots: a) PC _*š:a-_ 'hunt', _*š:a-ḳ^{w}a_ 'hunter' : Ub. _s^{w}ā-ḳ́a_ 'hunter' (= PC _*š:a-ḳ^{w}a_). This is a typical case of PWC *š̌:^{w} (in my reconstruction), that yields PC *š:, Ub. s^{w} and Abkh. *š. Another case of the same correspondence is: PC _*š:a_ 'fat', Abkh. _*š(ə̄)ša_, Ub. _s^{w}ə̄-q̇a_ 'butter, fat'; (b) PC _*śə̄-ħV_ 'deer' : Ub. _λa-š^{w}a_ 'hunting' : Abkh. _*š^{w}a-ra_'hunting, wild animal'. This is a case of PWC *š̌^{w} (in my reconstruction). Chirikba also reconstructs here *š̌^{w} (see below), mercilessly extracting Tapanta _š^{w}ə̄r_ 'wild animal' from the Common Abkh. root _*š^{w}ara-_ (/_*š^{w}ə̄rə̄-_) and comparing it alone with PC _*śə̄ħa_ on p. 266.
(2) PC _*ša-pχa_ 'measure' : Ub. _š^{w}a_ 'price, to value' : Abkh. _*š^{w}a-_ 'to measure (time, space)'. Ub. _š^{w}a_ should rather be compared with Abkh. _*š^{w}a-_ 'to pay' (see NCED 797), while PC _*ša-pχa_ 'measure, size, example' is hard to separate from _*šə̄-pa-_ 'collect, pick out one by one' and _*ša-ta-_ 'try out, check', the etymology of which see above.

(3) PC **šə̄g^{w}ə* 'mountain top' : Abkh. **š^{w}ə̄bə* 'hoar-frost' (??) The etymology of **šə̄g^{w}ə̄* (= Ub. *šaḳ^{w}a* id.) see above, p....
(4) Ub. *mə̄š^{w}ə̄* 'grapes' : Abkh. **lə̄-mə̄š^{w}ə̄* 'clematis' is a good match, but fits also into the correspondence "PC *ś : Ub., Abkh. *š^{w}", i.e. PWC *š̄^{w}.
(5) Ub. *š^{w}a-č̄a* 'to laugh' : Abkh. **pə̄-š^{w}ə̄-r-č̄əč̄a-*. This could also be a good parallel (with Ub. š^{w} = Abkh. š^{w} < PWC *š̄^{w}, just like in the preceding case); unfortunately Abkh. **pə̄š^{w}ə̄-rč̄ə̄č̄a-* means 'to laugh with lips' (Abkh. *a-pə̄š^{v}* 'lip'), while Ub. *š^{w}a-* = PC **-xə̄* in **č̄-xə̄* 'laugh' and goes back to PWC **ƛ̌^{w}V* (see above, p...).

As for the reconstruction of PWC *š^{w}, *š:^{w} - see above, pp....

PWC *š̄^{w} > PC *ś, *š̄ : Ub. š^{w} : Abkh. *š^{w}

This is basically the same row of correspondences (and the same reconstruction) as for *š̄^{w} in NCED p. 182. There are, however, minor differences:

a) the unmotivated split into *ś and *š̄ in PC is based on one parallel (PC **waš̄ə̄* : Ub. *ǵaš^{w}a,* Abkh. **-g^{w}aš^{w}* 'axe'), where PC *w- is also irregular. PC **waš̄ə̄* is most probably borrowed from an Iranian source (Osset. *wäs,* Old Ind. *vāśi-,* see Abayev 4).

b) Chirikba proposes to connect PWC **š̄^{w}ə̄-* 'deer' with PEC **chwōlĕ* 'fox' (NCED 324), rejecting the quasi-homonymous PWC root **š̄^{w}V* 'fox, jackal' proposed ibid. He treats Bzyp *a-š^{v}á-bga* 'red fox' as *aś^{v}ə̄-bga* 'Abaza fox'. I do not know Chirikba's sources in this case; the recording *a-š^{v}á-bga* is present in Marr's dictionary (but unfortunately absent in Bgazhba 1964). Cf. also Tap. *š^{w}a-gala* 'horde of wolves'.

c) this time Chirikba does not include in his correspondence the tense PC reflex. However, there exists a special correspondence of PC *š: to Ub. s^{w} and Abkh. *š which it is natural to treat as PWC *š̄:^{w}. Examples (**š̄:^{w}a* 'hunt', **š̄:^{w}a* 'oil, grease') see above, p... This is one more case of a specific external parallel for PC tense consonants, again disproving Chirikba's thesis of their secondary origin in Circassian.

Obstruent laterals

Chirikba reconstructs a rather defective system of lateral affricates, which includes:

ƛ ƛ̣
ƛ̌ Ł́
ƛ^{w}
ƛ̌^{w}

Here he again tries to ignore the reconstruction presented in NCED without giving any reason for it (let me note, however, that the correspondences for *ƛ̣ and *Ł́ are just the same as in NCED p. 182).

Above (pp....) I have given the reasons for reconstructing PWC *Ł, *Łʷ as well as labialized palatalized *ƛ́ʷ, *ƛ̣́ʷ, *ƛ́:ʷ and *Ł́ʷ. Let us now proceed to filling the other gaps (and make corrections) in the PWC system of lateral affricates.

1. PWC *ƛ, *ƛ́

Chirikba (p. 269) reconstructs *ƛ for the row of correspondences: "PC *š:, š : Ub. ś : Abkh. *x". He notes that I reconstruct a tense lateral fricative *λ: here (although it is not quite correct: I reconstruct *λ: for PC *š: : Ub. ś : Abkh. *x, but *λ (a lax lateral fricative) for PC *š : Ub. ś (the Abkhaz reflex here would be also *x, but no examples are known). He, however, says: "I opt for an affricate here, as I don't reconstruct tense consonants, and because the affricate corresponds nicely to the affricate in Proto-Avar-Andi word for 'three'". This is unusual for Chirikba, who does not frequently pay attention to the EC parallels.

The EC argument is of no value here, because PEC voiceless affricates usually give fricative reflexes in WC: cf. *c > s (~z), *cw > *šʷ, *ƛ > *λ, *q > *χ, *qw > *χʷ, see NCED 45-58; the only exceptions are when they are followed by long vowels (and thus give tense reflexes: *c:, *c:ʷ etc.), and the case with middle and back affricates (*ć, *č) that are not fricativized. From within WC the correspondence "š:, š : Ub. ś : Abkh. x" certainly looks like reflecting a fricative. Moreover, there is a very good candidate for PWC *ƛ in the row:

PC *č : Ub. š : Abkh. *x

Chirikba (p. 270) reconstructs here PWC palatalized *ƛ́. However, he fails to notice (or recognize, because it is stated in NCED 182) another row of correspondences:

PC *č : Ub. š : Abkh. *x́

Cf. PC *čə̄-* 'to milk' : Ub. *ša-* id. : Abkh. *x́a-* id.
Ad. *(šχʷa-)mč̃* 'chestnut' : Ub. *ś̌-xə̄* id. : Abkh. *x́a* id.
PC *čə̄-* 'to breed, give birth (to animals)', *čə̄-rə̄* 'young (of animals)' : Ub. *ša-, ša-da-* 'give birth (to animals) : Abkh. *x́a-* id.

The latter correspondence looks exactly like the palatalized correlate of the former, thus we can safely reconstruct *ƛ for "PC *č : Ub. š : Abkh. *x" and *ƛ́ for "PC *č : Ub. š : Abkh. *x́".

2. PWC *ƛ:

In the word for 'night' on p. 269 (PC _*č:aš:ə̄,_ Ub. _śə̄śə,_ Abkh. _*cə̄xə_) Chirikba reconstructs _*cə̄ƛə̄,_ supposing an assimilation (_*caš:ə̄_ > _*č:aš:ə_) in PC and Ubykh (_*cə̄śə̄_ > _*śə̄śə_). He himself mentions, however, PC _*nə̄-č:a-p:a_ 'this night', where no conditions for assimilation exist and which still has *č:. For me the solution is obvious: the correspondence PC *č: : Abkh. *c can only point to a tense lateral affricate *ƛ:. The correspondence is, in fact, absolutely parallel to PC *č̣ : Abkh. *c̣ < PWC *Ł, see p... In Ubykh one should have rather expected ć (cf. *Ł > ḉ), but here it may be indeed due to assimilation (*ćə̄śə̄ > śə̄śə̄).

3. PWC *ƛ̣

The correspondences for velar voiced fricatives in WC languages are "PC *ɣ : Ub. ɣ / ʁ : Abkh. *ɣ" for *ɣ and "PC *ž : Ub. ź : Abkh. *ɣ́" for *ɣ́, see p... Several roots, however, reveal a somewhat different correlation of PC *ɣ and Abkh. *ɣ́ (Ubykh here can have either ʁ or ź). These are the cases:

PC _*pa-ɣá_ 'proud' : Ub. _ʁa-bə̄_ 'hard' : Abkh. _*baʁa_ 'hard; niggardly'
PC _*ɣə̄-_ in _*ɣə̄-bza_ 'swear, damn' : Abkh. _*ʁə̄_ 'swear, curse'
PC _*ɣa_ 'testiculi' : Ub. _-źa_ 'male' : Abkh. _*ʁa_ 'male'

The Abkh. reflex (*ɣ́) here reminds of the development *ƛ > *x́ (see above), and in two of these cases we have EC parallels with a lateral (_*ƛ̠wĕrV_ 'hard', see NCED 792, and _*liwƛ̣ĕ_ 'man, male', see NCED 749). It seems reasonable, therefore, to reconstruct a voiced palatalized affricate (*ƛ̣) here. Note that we can not reconstruct a fricative in this case, because for PWC *Ľ there exists a well established correspondence (PC *L : Ub. *L : Abkh. *ž).

4. PWC *ƛ^{w}, *ƛ:^{w}

Here Chirikba follows the reconstruction presented in NCED, however, again merging lax *ƛ^{w} (> PC *č) and tense *ƛ:^{w} (>PC *č:).

5. PWC *ƛ̣^{w}

Chirikba accepts my reconstruction of *ƛ̣ (> PC *tħ, Ub. L, Abkh. l); however, he fails to see that the well known correspondence "PC *ħ : Ub. w : Abkh. l" is looking very much like the labialized counterpart of *ƛ̣. It occurs in several roots:

PC _*ħa_ : Ub. _wla_ : Abkh. _*la_ 'dog'
PC _*ħa_ : Ub. _wə̄-_ : Abkh. _*-lV-_ 'enter'
PC _*ħa-ga_ : Abkh. _*la-ga-_ 'to grind' (cf. also _*lə̄wə̄_ 'grinding stone')
Ub. _wasə̄_ : Abkh. _*lasə̄_ 'light'
Ub. _š^{w}ə̄-wa_ : Ad. _*-śħa_ 'night' (see above).

Some of them also have EC counterparts with laterals (for _*ƛ̣^{w}a_ 'enter' : PEC _*=ə̠rƛU,_ see NCED 422-423; _*-ƛ̣^{w}a_ 'night' : PEC _*rVmƛ̠A_).

As a result it can be seen that PWC in fact possessed an almost full system of lateral affricates:

*ƛ	*ƛ:	*ƛ̣	*Ł
*ƛ́	[*ƛ́:]	*ƛ̣́	*Ł́
*ƛ^w	*ƛ:^{w}	*ƛ̣^{w}	*Ł^w
*ƛ́^{w}	*ƛ́:^{w}	*ƛ̣́^{w}	*Ł́^{w}

Lateral fricatives

I have nothing to add to Chirikba's reconstruction of *Ľ, *ƛ́, *λ^w and *ƛ́^{w} : these correspondences coincide exactly with those formulated in NCED (pp. 182-183).

As for *λ and *λ:, I have shown above that they must be reconstructed instead of Chirikba's *ƛ. Chirikba has a phoneme *λ present in one word (PC **pλə̄-,* Ub. *pλa-,* Abkh. **pə̄šə̄-* 'to look'), but I think that this is a result of variation in labialization (see NCED 1031, where the root is compared with EC **=VrλwEn* 'to see, look'): PWC **pə̄-ƛa-* (without the prefix cf. PC **λa-ƃ^{w}ə̄-* 'to see') > Ub. *pλa-,* PC **pλa-,* but PWC **pə̄-ƛ^{w}a*> Abkh. **pə̄šə̄-.*

Additional remark: Chirikba gives a rather strange comment on p. 276. He says: "Nikolayev and Starostin reconstruct PWC **maλwV* 'day', i.e. with the sequence λw, rather than with the labialised λ^w, which I don't find convincing." This is, of course, an unfortunate misprint instead of **maλ^{W}V* - but every person who has read through the introduction and the description of WC phonology in NCED, should be able to understand that nothing like "sequence λw" was ever reconstructed in any WC protoform. Further, Chirikba protests against our separating PC **max^{W}a* 'day' and 'luck'. He says that "the meaning 'happy' might have evolved from a lost idiomatic expression containing the word 'day'". I would be grateful if he could produce such an expression; until that is done, I will prefer to compare the (fortuitously) homonymous **maλ^{W}V* 'day' and **maλ(ʹ)^{W}V* 'luck' with EC **mīλwĂ* 'sun, day' and **wēnλ̱wē* 'luck' respectively.

Velar fricatives

Velar fricatives *ɣ, *x and *x^w in Chirikba's system are the same as in NCED (p. 183). Our systems differ, however, in what concerns palatalized velars.

On pp.... above I have proposed a *ɣ́ for the correspondence "PC *ž : Ub. ź : Abkh. *ɣ́", and a *ƛ̣́ for the correspondence "PC *ɣ : Ub. ź / ƃ : Abkh. *ɣ́". Chirikba does not acknowledge the first correspondence, while reconstructing *ɣ́ for the second one.

This is one of the cases when it is rather hard to draw conclusions on the WC

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data solely, and external parallels help. We have seen above, that there are several cases of words with the latter correspondence having lateral consonants in their EC cognates. It is, therefore, very probable that palatalized *ɣ́ should be reconstructed for the first correspondence, and palatalized *ƛ̣ for the second one.

Parallel to the correspondence "PC *ɣ : Ub. ź : Abkh. *ɣ́" is its voiceless counterpart: "PC *š́ : Ub. ś/x́ : Abkh. *x́". This is a rare phoneme, known to me in three examples:

PC _*š́tə̄-_ ’to freeze’ : Abkh. _*x́ə̄-_ ’to freeze, become cold’, _*x́ə̄-_ _ta_’cold’
PC _*pšə̄-sa_ / _*psə̄-sa_ (dissimilation < _*pš́ə̄-sa-_ ?) ’tale’ : Ub. _mə̄śa_’to call, read’ :
  Abkh. _*pə̄x́a-_ id. (see above)
PC _*š́ə̄-pa_ ’first, for the first time’ : Abkh. _*pə̄x́a_ ’earlier, before’:
  Ub. _fā-χ́a_ id. (cf. also Abkh. _-χ́a_ in _ja-χ́a_ ’today’, Ub. _č̣a-χ́a_
  ’today’, _š_^{W}_a-χ́a_ ’this year’).

Chirikba does not accept this correspondence, but instead produces his own one:

PWC *x́ > PC *x : Ub. š́ : Abkh. *x́

Here in two examples (No 4 and No 5) Chirikba omits the Circassian parallels (for Ub. _š́a,_ Abkh. _*x́a-_ ’to milk’ we have PC _*č́ə̄-;_ for Ub. _š́a-da-,_ Abkh. _*x́a-_ ’to give birth (to animals)’ we have PC _*č́ə̄_-id.). It is therefore clear that these are cognate sets actually reflecting PWC palatalized *ƛ́, see above, pp....

In No 6 Chirikba compares PC _*č-xa-_ ’to eat’ with Abkh. _*x́a_ ’provisions’ (known only in compounds); as I have written above, I think a much better match is the usual Abkh. root for ’eat’, _*fa-_ (< PWC _*fV-,_ see above, p....). We are therefore left with two grammatical suffixes (No 2 and 3), which, unfortunately, are rather often subject to irregular changes, and No 1: PC _*pxa_ : Ub. _pš́a_ : Abkh. _*pə̄x́a_ / _*x́a_ ’back part’. The Abkhaz root does indeed have the meaning ’back’ in several verbal compounds, but it is very difficult to separate it from _*pə̄x́a_ ’earlier, before’ (= PC _*š́ə̄-pa,_ see above). Thus here we have, in fact, just a case of PC *x : Ub. š́ pointing to PWC *x, see above.

The correspondence that Chirikba proposes for PWC *x́^{w} (PC *x^{w} : Ub. x : Abkh. *x́) is also very dubious. There are just two examples:

(1) PC _*x_^{w}_a_ ’to fit, find place in’ : Ub. _xa-_ ’to be (pl.)’ : Abkh. _*xa-_ ’to stay, remain’.

As Chirikba notes himself, PC _*x_^{w}_a-_ is actually the same as _*x_^{w}_a-_ ’to fall’ (cf. Russian _падать_ ’fall’ : _попадать_ ’to get, fit into’). This root corresponds to Ub. _χ́a-_ ’to fall’ and to Abkh. _*s_^{w}_a-_ ’to fall; to fit into’ (used with preverbs). The same correspondence (PC *x^{w} : Ub. χ́ : Abkh. *s^{w}) is found in: PC _*x_^{w}_a-_ ’in favour of’ (preverb) : Ub. _χ́a-_ id. PC _*tx_^{w}_a-jə̄_ ’beech-tree’ : Abkh. _*s_^{w}_ə̄_ id.

It is natural to reconstruct a palatalized labialized *x́^{w} for this correspondence.

As for the suffix PC _*-x_^{w}_a_ ’leftovers’ : Ub. _-xa_ id. : Abkh. _*-xa_ id. (treated by Chirikba under the same root), as well as No 2, Ub. _-xa_ ’time for doing smth.’ :

Abkh. **-xa* id., **a-xa* 'time', here we may deal with an irregular delabialization in Abkhaz (note that both roots are suffixed auxiliary morphemes easily liable to changes like that). In that case both examples can be treated as PWC *x^w, with sporadic delabialization (which is actually quite frequent in the reflexes of this phoneme, not only in Abkhaz, but in Circassian as well).

There is also one rather reliable lexical example which seems to represent the voiced correlate of *$x́^w$, namely *$ɣ́^w$ - with the reflexes PC *$ʁ^w$ (note that no *$ɣ^w$ exists in PC) : Ub. ʁ́ : Abkh. *z^w. This is the numeral "nine": PC **bʁʷə* : Ubykh *bʁ́ə̄* : Abkh. **zʷə̄-*.

Thus we are able to reconstruct an almost complete system of PWC velar fricatives:

*x *ɣ
*x́ *ɣ́
x^w [$ɣ^w$]
*$x́^w$ *$ɣ́^w$

Uvular and pharyngeal fricatives.

Above (in the section concerning uvular stops / affricates) I have already mentioned the basic different points between Chirikba's system and mine: a) where Chirikba reconstructs voiced uvular fricatives (*ʁ, *ʁ́, *$ʁ^w$), I reconstruct voiced uvular stops (affricates) *G, *G, *G^w; b) where Chirikba reconstructs voiced pharyngeals (= emphatic laryngeals) *ʕ, *ʕ, *$ʕ^w$, I reconstruct voiced uvular fricatives (*ʁ, *ʁ́, *$ʁ^w$).

The reason here is obvious: I am unwilling to suppose an extremely unusual development of the type *ʕ > ʁ, while the other direction (*ʁ > ʕ) is very common in many languages of the world.

There are, however, some other differences that I shall deal with below.

1. In NCED I have not reconstructed pharyngealized *GI for PWC. Two of Chirikba's comparisons, however, suggest a possibility of reconstructing *GI (> PC *ʁ, Ub. ʁI, Abkh. *ʁ) in the roots **GIa* 'to widen' (PC **ʁaʁa-* 'to flower, bloom' : Ub. *ç̌a-ʁIa-* 'to yawn' (*'open the mouth'; on Abkh. **ʁaʁa* 'spacious; fat, plump' see, however, above, p...) and **GIV-* 'to cut' (Ub. *q̇Iə-ʁIə̄-* : Tap. *a-ʕʷə̄-r-ʁʁa-*). Chirikba, of course, says that Ubykh pharyngealization here is 'expressive', which I doubt (see the discussion above). Likewise, a labialized *GI^w (absent in NCED) may be reconstructed in **GIʷaGIʷa-* 'sad, lonesome' (PC **ʁʷaʁʷa* : Ub. *ʁIʷaʁIʷa-* 'to huddle oneself, shrivel' : Abkh. **ʁʷəʁʷə̄-*).
2. Chirikba reconstructs a palatalized labialized *$ʁ́^w$ (which would be *G^w in my system) in the root for 'nine', where I have reconstructed a velar *$ɣ́^w$ (see above): note that the apparent orthographic identity of *$ɣ́^w$ in NCED and *$ɣ́^w$ in Chirikba's

book is deceptive (because his symbol /ɣ/ = /ʁ/ in NCED, while his /ĝ/ = /ɣ/ in NCED). There is, however, one good case of *Gʷ which Chirikba ignores: PC **cʷə̄ʁʷa-* 'to envy' : Ub. *cʷə̄mʁə̄-* : Abkh. **cʷə̄mə̄ʁə̄- < *cʷə̄mə̄GʷV-*.

3. The reconstruction of voiceless uvular fricatives in Chirikba's book has to be discussed specially. Here his basic difference from NCED is the innovative idea that PWC *χ (as well as *χ́, *χʷ, *χ́ʷ) have not yielded emphatic laryngeals (*ħ, *ħʷ) in Abkhaz, but changed into velars (*x, *x́, *xʷ) [Chirikba treats them as uvulars - since there is no distinction between velar and uvular fricatives in Common Abkhaz]. In their turn, the sets of correspondences where Abkhaz has *ħ, *ħʷ, are treated as reflecting PWC emphatic laryngeals (pharyngeals) *ħ, *ħ́ etc., once again supposing an unnatural change of the type *ħ > χ. We shall have to discuss the individual sets of correspondences here.

1. PWC *χ > PC *χ : Ub. χ, χI : Abkh. *x

(1) PC **χa-* 'to knit, weave' : Ub. *χa-* 'to knit' : Abkh. **xVxa-* 'to spin thread'. It is not clear to me why Chirikba proposes this Abkhaz parallel instead of the perfectly regular Abkh. **ħa-* 'to knit, weave'.

(2) PC **χə̄rə̄* : Ub. *χə̄r* : Abkh. **xə̄rə̄-xə̄rə̄* 'to snore'. I shall not discuss the validity of this example.

(3) PC **ʁa-xa-* 'to do, accomplish' : Abkh. **xa-* id. The match seems good, but it was difficult for me to identify the source of the Abkhaz reconstruction - before I looked into Chirikba 1996 and found that **xa-* is reconstructed on the basis of Tap. *xa-* - a word recorded in the Abaza village Gumlokt (with 2 isolects), apparently by Chirikba himself. No other Abkhaz dialectal recording or dictionary shows such a word, so its validity is extremely dubious (a Circassian loan?).

(5) Kab. *χa-ra-χa-* 'to be empty-headed, giddy, fussy, fidgety' : Ub. *-χIa* in *bzə̄-χIa* 'pouring rain' : Abkh. **xə̄xə̄-* (in comp.) 'to run up to, hasten to'. I can only place a big question mark (?) after all these "cognates".

(6) PC **pχə̄-* : Ub. *pχa-* 'to strew, scatter'. Here there is no Abkhaz match, and in my system it is a perfect example of PWC *χ.

(8) PC **č̣:a-pχə̄-* 'to dry up, dry out' : Abkh. **pə̄xa-* 'warm'. Despite Chirikba, it is absolutely impossible to separate Abkh. **pə̄xa*'warm' from Ubykh *pś̄ə̄* 'warm' and PC **pš̄-ta-* 'boil' with a perfectly regular reflex of PWC **pə̄λV*.

We are left with two examples that seem plausible to me:

(4) Ub. *χIa-* 'to scrape, comb, scratch' : Abkh. **xaxa-* 'to scratch itself (as a dog)'. For the Ub. form cf. also PC **tχa-* 'to scratch'.

(7) PC **-χa* 'scant, wide-spaced' (in compounds) : Ub. *tχa* 'thin, sparse' : Abkh. **xaxa-* 'scant, thin'.

The irregular development in Abkhaz here (x instead of the expected ħ) can be explained by a special development within a cluster (cf. Ub. tχ-, PC *tχ-), see below.

The normal correspondence (see NCED 183) is "PC χ : Ub. χ : Abkh. ħ", cf.:

PC *χa- 'to knit' : Ub. *χa-* id. : Abkh. *ħa- 'to weave, knit' (see above)
PC *λa-χa 'fetter' : Ub. *λa-χ* id. : Abkh. *š̄a-ħa- id.
Ub. *baχá-* 'to be angry' : Tap. *baħa-* id.

2. PWC *χ́ > PC *χ : Ub. χ́ : Abkh. *x́

Here Chirikba lists four examples, stating himself that the first (Ub. *-χ́a* 'place of' : Abkh. *a-x́ə̄ 'towards, to') and the second (Ub. *χ́a-* pref. 'towards' : Abkh. *a-x́ə̄ 'adverb. prefix of reason') are probably etymologically identical. They are also most probably identical with No 3 (Ub. *-χ́a* in *č̣a-x́a* 'today', *š^{w}a-χ́a* 'this year' : Abkh. *-x́a* in *ja-x́a* 'today'), and neither one has a Circassian parallel. Now I think that the PC parallel is *š̄ə̄- in *š̄ə̄-pa- 'for the first time' and the root actually reflects PWC *x́V (see above).

As for the fourth example (PC *tχa- 'to scratch' : Ub. *tχ́ə̄-* 'to tear'), here the PC form corresponds in fact to Ub. *χIa-* 'to scratch' (see above) < PWC *(T)χIa-.

The correspondence "*χ : χ́ : *x́" thus does not exist. The normal correspondence for PWC *χ́ is "PC *χ : Ub. χ́ : Abkh. ħ", which Chirikba accepts (p. 298-299), but reconstructs PWC *ħ́.

3. PWC *χ^{w} > PC *χ^{w} : Ub. χ^{w} : Abkh. *x^{w}

The examples given are again absolutely heterogeneous.

(1) Here PC *χ^{w}až̄ə̄- 'to change' is compared with Ub. *χ^{w}a-da-* 'buy' and Abkh. *x^{w}a- 'to take, buy' (used with prefixes). As a matter of fact, PC *χ^{w}až̄ə̄- has a perfect match in Tapanta *ħ^{w}aǯə̄-* 'change', going back regularly to PWC *χ^{w}aǯə̄- (with the standard correspondence PC *χ^{w} : Abkh. *ħ^{w}; loan in this case is impossible), while Abkh. *x^{w}a- 'buy', *x^{w}ə 'price' corresponds regularly to PC *x^{w}ə̄- 'buy' (see NCED 842, with a more questionable Ubykh parallel: Ub. fa- 'to pay') < PWC *x^{w}V-.

(2) Here PC *-(p)χ^{w}a- in *ʔa-(p)χ^{w}a-mba 'finger', *λa-(p)χ^{w}a-mba 'toe' is compared with Abkh. *-x^{w}a in *ma-x^{w}a 'arm', *š̄a-x^{w}a 'shin-bone', 'marrow'. The latter root, however, had been long ago (see Klimov 1967) identified (with the meaning change 'joint' < 'sinew') with PC *x^{w}a 'vein, sinew, blood vessel' < PWC *x^{w}a.

(3-4) PC *jə̄-ɓa-χ^{w}a- 'to pour in' : Ub. χ^{w}a- 'to tack, baste thread' : Abkh. *χ^{w}a- id. From Circassian one should rather take here PC *χ^{w}ə̄-p:q:ə̄ 'weaving loom; form or frame for modelling clothes'. The latter is compared by Chirikba in No 4 with Abkh. *x^{w}a 'pivot of the shuttle' - the original meaning of which is, however, just 'handle', preserved in Abaza (and probably identical with *x^{w}a 'joint' in No 2). I would prefer to compare PC *χ^{w}ə̄- with Ub. χ^{w}aand Abkh. *ħ^{w}a- 'to bind, plait' (cf. further PEC *=irχwVn 'to knit, weave, spin', NCED 655). Abkh. *x^{w}a- 'to tack, baste' should be probably regarded as an old cultural loanword.

(5) Here the PC *-χ^{w}ə̄ and Ub. -χ^{w}a, a component of some words with the vague meaning 'big' is compared with Abkh. *x^{w}ə̄x^{w}a 'long, tall, slender, prolonged'. Most

uncertain (several alternative etymologies - equally uncertain - can be proposed both for PC / Ub. and Abkh.).

(7-9) Here only PC forms are compared with Ubykh, thus giving us perfectly regular patterns of the correspondence "PC *χ^{w} : Ub. χ^{w}", but no Abkhaz parallels are given.

(10) Ub. bIašχ^{w}ə̄ 'wild grape' : Abkh. *šχ^{w}ə̄ç̣^{w}ə̄ 'husked corn-cob' (?) Extremely dubious both phonetically and semantically.

This leaves us with example No (6) - PC *tχ^{w}ə̄, Ub. tχ^{w}ə̄ : Abkh. *x^{w}ə̄ša 'butter'. I have rejected this comparison in NCED (948), but now I am not so sure. If we compare the three exceptions from the standard rule "PC *χ, *χ^{w} : Ub. χ, χ^{w} : Abkh. *ħ, *ħ^{w}", namely: PC *tχa- 'scratch' : Abkh. *xaxa-; Ub. tχa 'thin' - Abkh. *xaxa and Ub. tχ^{w}ə̄ 'butter' : Abkh. *x^{w}ə̄-ša, we may be able to formulate a more general rule: PWC *χ > Abkh. *ħ, but PWC *Tχ > Abkh. *x- without laryngealization.

The normal correspondence for *χ^{w} is "PC *χ^{w} : Ub. χ(^{w}) : Abkh. ħ^{w}" which Chirikba accepts (p. 299-301), but reconstructs PWC *ħ^{w}.

4. PWC *χ́^{w} > PC *χ^{w} : Ub. x(^{w}) : Abkh. *x́

Here only one example is given, namely, the root for 'chestnut' which was treated above (p...) and was shown to have contained a lateral *ƛ. Here in the Ubykh form (š̌xə̄) it is š̌ that corresponds to Abkh. *x́ (*a-x́a) and not x! Also, the PC compound *šχ^{w}a-mč̌ə̄ contains *šχ^{w}a- 'grass' or 'grey', and it is the part *-mč̌ə̄ that corresponds to Ub. š̌(-xə̄) and Abkhaz *(a)-x́a. This is all a complete misunderstanding.

Again, the normal correspondence here is "PC *χ^{w} : Ub. χ́ : Abkh. ħ^{w}". Chirikba accepts it (p. 302-303), but reconstructs PWC *ħ̆^{w}.

It is clear that most of the examples that Chirikba gives to demonstrate the reflexes PWC *χ > Abkh. χ etc., are invalid - except for a few roots that probably contained a consonant cluster like *Tχ-.

Pharyngealized uvulars.

While discussing the system of uvulars, we have seen that some Ubykh pharyngealized consonants have specific correspondences in other languages (despite the claim made by Chirikba that Ubykh pharyngealization is always secondary).

Uvular pharyngealized fricatives normally have the same correspondences as the non-pharyngealized ones, as we have seen above: cf. *TχIa- 'scratch' (see above), as well as some other examples (treated by Chirikba as pharyngeals): *pə̄χIa- 'to rush at' (PC *pχa- : Ub. pχIa-); *χI^{w}ə̄- 'to graze' (PC *χ^{w}ə̄-, Ub. χIə̄-, Abkh. *ħ^{w}ə̄-);

*χIʷV- 'to crawl' (PC *cʷa-χʷa-, Ub. χIʷəχIʷ-da, Abkh. *ħʷa-za-); *χIʷə- 'to rotate' (PC *ḳara-χʷa-, Ub. ča-χIʷə-, Abkh. *ħʷə-); *χIʷə- 'to rob' (Ub. χIə-cʷa 'finder of a lost animal or stolen girl' : PC *χʷə-nc̣a- 'rob' : Abkh. *ħʷə- 'rob'); *ɓIV- 'to be born, arrive' (Ub. ɓIə- : Abkh. ʕa-); *ɓIə- 'to howl' (PC *ɓə- : Ub. ɓIə- : Abkh. *qʷa-ʕə-); *bIəɓIa- 'barren, thin' (PC *bɓa : Ub. bIɓIa : Abkh. *abʕa); *bIəɓIa 'eagle' (PC *bɓa : Ub. bIɓIaǯá); *mIəɓIʷa 'evil, unlucky' (PC *məɓʷá : Ub. məɓIʷá).

On the analogy with the system of affricates one would also expect in PWC the presence of palatalized pharyngealized (*χ́I, *ɓ́I etc.). And in effect there are several rows of correspondences that Chirikba for the most part treats as "irregular", but which are perfectly well explained if we assume the presence of pharyngealized palatalized uvular fricatives:

a) PWC *χ́I > PC *ħ : Ub. χI : Abkh. *ħ

Chirikba acknowledges this correspondence, reconstructing here his pharyngeal *ħ (p. 297) (and thus having to reject the examples with PC *χ : Ub. χ : Abkh. *ħ, see above). Following his principle of the "secondary nature" of Ubykh pharyngealized phonemes, he writes here: "Ub. χ, χI" with a comma. It is, however, very easy to notice that in all cases when we actually have here a Circassian parallel (with *ħ), Ubykh has only χI, cf.:

PC *-ħa 'suffix denoting the action occurring in cycles, rounds' :
Ub. χIə-χIə 'by rounds, circularly'
PC *ħa(n)čə 'scoop' : Ub. χIaš 'spade, shovel'
PC *ħama 'threshing floor' : Ub. χIəmə 'harvesting'
Ub. cʷə-χIa- 'to curse' : Abkh. *cʷə-ħa- id. (Chirikba does not list a PC parallel, but cf. perhaps PC *čə-ħa- 'to provoke smb.')

All the examples with Ub. χ here are actually the matches of Ub. χ and Abkh. *ħ, perfectly explained by PWC *χ (in my system, see above):
Ub. baχa- : Tap. baħa- 'be angry, sulky' (see above)
Ub. χaχa- 'be astonished, surprised' : Abkh. *ħa- 'become afraid'
Ub. ṭχara 'break in small pieces' : Abkh. *təħara-ħʷa 'rhythmical (of heart's beating)' [which is, however, a rather unfortunate example]

There is not a single case of "PC *ħ : Ub. χ" - which means that here again we deal with a special PWC pharyngealized phoneme, which I reconstruct as *χ́I.

b) PWC *ɓ́I > PC *j : Ub. ɓ : Abkh. *ʕ

This is the correspondence in the numeral "eight" (PC *jə : Abkh. *a-ʕə), classified by Chirikba as "irregular" (p. 295). Ubykh unexpectedly has here ɓʷ (Ub. ɓʷa), which may be explained by an old analogy with the next numeral, PWC *bγ́ʷə (see above).

Another example may be *ɓ́Iə- 'evil; guilt' (Ub. ɓə-, Abkh. *ʕa- 'guilt'; PC *-jə suffix meaning 'bad').

c) PWC *ʁIʷ > PC *w / *ʁʷ : Ub. w : Abkh. *ʕʷ

This correspondence (with several examples) is also accepted by Chirikba, but treated as "irregular" (p. 294). It is observed in *ʁIʷV 'metal, copper' (PC *ʁʷə̄, Ub. wə̄-, Abkh. *(bə̄)ʕʷa), *ʁ́Iʷə̄na 'house' (PC *wə̄na, Abkh. *ʕʷə̄na) etc.

The following system of uvular fricatives can therefore be reconstructed:

χ	ʁ	χI	ʁI
χ́	ʁ́	χ́I	ʁ́I
χʷ	ʁʷ	χIʷ	ʁIʷ
χ́ʷ	ʁ́ʷ	[χ́Iʷ]	ʁ́Iʷ

Resonants

Most resonants (*m, *n, *w, *j) have not changed at all since PWC; they give identical reflexes and are identically reconstructed by Chirikba and in NCED.

There are, however, some differences in the reconstruction of liquids. In NCED I reconstruct a contrast of palatalization for liquids, thus reconstructing a distinction of *r - *ŕ and *l - *ĺ. Chirikba sticks to 2 liquids (*r and *l) which is certainly simpler - but again he has to admit a lot of secondary unmotivated splits (thus *r > PC *d-, *t:-; Ub. L-, d-; Abkh. *r-, l-) which certainly does not look convincing. However, there are not many examples of initial liquids, and it is rather hard to argue in favour of particular solutions.

Winding up this rather lengthy review - which I, in fact, have used to elucidate some points of WC reconstruction that were only briefly mentioned in NCED - I should say that, despite my disagreement with many of Chirikba's conclusions, I have read the book with satisfaction. The author clearly supports the idea of Common North Caucasian. He also thinks that Common West Caucasian can be reconstructed (despite the skepticism of some of our predecessors). He adds a significant number of new WC lexical matches, and among his valid phonological propositions I can name: the reconstruction of CA *v; the reconstruction of PWC *q́; the PC reflex *ʁ of PWC *G which was hitherto unknown; the development of PWC *tχ > CA *x. The book undoubtedly will stimulate further research in the West Caucasian and North Caucasian areas.

Abbreviations

CA - Common Abkhaz
CWC - Common West Caucasian
PEC - Proto-East-Caucasian
PNC - Proto-North-Caucasian

PC - Proto-Circassian
PWC - Proto-West-Caucasian
WC - West Caucasian

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